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XVIII. *The Foundations of Dynamics. By* OLIVER LODGE, *F.R.S., Professor of Physics in University College, Liverpool**.

PART I.

The Nature of Axioms.

IT is a matter of congratulation with me that a critic who has devoted so much thought to Newton's laws of motion and similar fundamental doctrines should have begun a discussion of my papers on the subject of Energy ; and I shall willingly consider his objections in order to see what modifications, if any, should be made in my original statements. But Dr. MacGregor's temporary attitude towards Physical problems is exhibited rather strikingly in a treatise on " The Fundamental Hypotheses of Abstract Dynamics," which he published as a Presidential Address to a section of the Royal Society of Canada (Transactions 1892). Hence, before replying to his criticisms on my writings, as made in the February number of the Philosophical Magazine (vol. xxxv. p. 134), I should like to make a few general observations suggested by this other deliverance of his, so as to indicate what seem to me the rather different points of view from which we, or if not we some other writers, approach these funda-

* Read May 12, 1893.

mental doctrines of Mechanics and Physics. The difference in attitude may be briefly summarised thus :—Some philosophers seek to advance truth by detecting or inventing complications in what was apparently simple ; whereas others aim at making simple statements concerning things which are apparently or really complicated. A generalization like this is not one which will bear pressing into individual cases, but if it contains an element of truth it has reference to no personal detail, as it seems to me, but to a difference in type ; and I sometimes think that most minds, except those few of the very highest order who are above classification, may be said to fall into, or at least to lean toward, one or other of these categories *. Each type of mind performs its service, and each type has its appropriate danger.

The detection of a real complication is a service to truth ; the invention of a needless complexity is a disservice and temporary obstruction. The reduction of apparently complex facts to a simple statement in commonplace language is, I believe, a service ; the over-simple and incomplete summary of what is really complex is not an equal service, but I do not perceive that it is likely to be any serious obstruction : it seems to me rather of the nature of a first approximation, which is often temporarily helpful.

When Ohm stated his law that current is proportional to E.M.F., he did not know that it was really true. It has turned out to be precisely true for copper and for sulphate of copper—the only substances for which it has been seriously tested ; but even if it had not been so accurate, its statement

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* I see no reason in Dr. MacGregor's book on Dynamics for including him in the first category : it is his Presidential Address on the Laws of Motion that alone suggested it. I do not intend the classification as in any way offensive : I should think that Prof. Karl Pearson, for instance, would willingly enrol himself under the first head rather than under the second, judging by his 'Grammar of Science.' But very likely MacGregor has stated the laws of motion in their simplest conceivable form if attraction and repulsion across a distance are to be contemplated. That is the essential difference between us : he is willing to base Physics on action at a distance ; I am not. From the action-at-a-distance point of view his statements are in many respects admirable, especially those near the conclusion of his essay. The remarks in the text are intended to have only a general and impersonal application.

was a service, since it enabled half a century to walk in the light instead of in the dark. There is no evidence that it is accurately true for every variety of solid and liquid conductor, but by this time it is the fashion to assume its truth in ordinary simple cases. And rightly so, as it seems to me ; the burden of proof rests now with the enterprising experimenter who can detect a flaw in it. His evidence will be listened to, but till it is forthcoming vague doubts can be legitimately ignored.

Take another example :—The characteristic equation of gases in the simple form $pv = RT$ has done good service, though it turns out to be untrue for every actual substance. Without it, however, we should have been unnecessarily floundering in the dark. Even now it is more used in dealing with gases than any other equation. The improved statement of Van der Waals, adding a term to p and subtracting one from v, was another distinct service, and enabled a mass of experimental evidence concerning the structure of liquids to be conveniently and simply summarised. In its turn, however, it has had to give place to more complex empirical statements, and the complete law has not yet appeared.

The examples I have chosen, one of a precise, the other of an approximate, simple statement, are not indeed of the nature of axioms; and it may be held that it would be unsatisfactory to base our axioms on such a tentative sort of footing.

And yet what other course is open ? Truly axiomatic statements can only be effectively made concerning things of which the race has had a long course of experience,—things to which they have grown familiarly accustomed. If they can be actually proved, they are theorems, not axioms.

The setting forth of an axiom I regard as a kind of challenge, equivalent to the statement:—"Here is what seems to me to be a short summary of a universal truth ; disprove it if you can. I cannot prove it, it is too simple and fundamental for proof, I can only adduce hundreds of instances where it holds. I have indeed critically examined a few special cases and never found it fail, but a single contrary instance will suffice to overthrow it; hence, though it be hard to prove, yet if not true its disproof should be easy : find that contrary instance if

you can." If no disproof is forthcoming for a few generations, the axiom is likely to get accepted. Meanwhile its undeniable simplicity is a practical advantage, even though in the course of centuries a flaw or needful modification in its statement may be discovered.

This is the kind of basis on which such a law as that of the Conservation of Energy or the Conservation of Matter rests. That the perpetual motion is impossible, that matter is indestructible, that energy never diminishes in the act of transfer, all these must be regarded as generalizations based on a great series of experiments, some consciously directed to the upsetting of one or other of the laws if possible, some aimed at establishing them, but most of a non-contentious and collateral character. If we are challenged to produce direct evidence that in any given chemical reaction the mass of the reagents is unchanged, not only in the initial and final stages but at every stage of the process, the proof may be exceedingly difficult. Heat is liable to be developed which would interfere with delicate weighings, and the reaction challenged may be an explosive or otherwise inconvenient one. But we do not attempt the proof, we shift the burden on to the shoulders of the doubter and say to him, Disprove it if you can ; and so we practically say for all our axioms, and for all laws which are so simple and fundamental as to be hardly distinguishable from axioms.

Experiments are often made or adduced in support of a law as if they were part of its foundation : thus Newton tried experiments on impact before stating his third law, but the experiments did not really prove it with accuracy even for the particular case examined. All they could show was that there was nothing obviously wrong with it. He saw no reason for supposing it wrong, and so after consideration stated it as an axiom, to be hereafter challenged and found inaccurate if so the progress of experience turned out.

I should say that an axiom or fundamental physical law is a simple statement, suggested by familiar or easily ascertained facts, probable in itself, readily grasped, and not disproved or apparently liable to disproof throughout a long course of experience.

If a statement is capable of exact examination and veri-

fication, either by reasoning or by experiment, it is called a law, but not a fundamental law; *i. e.* it is no longer part of the foundation, it is supported on something else. If it has no support, except the absence of evidence against it, it is an axiom. Far be it from me to decry the use of experiments of verification. The necessity for them whenever feasible is conspicuous and universally admitted, and much ingenuity may be usefully spent upon them ; but I do say that in time a theory can become established by processes other than direct experimental verification; and in fact that really valid and flawless experimental verification is frequently an impossibility.

An instructive example of the legitimate strength of a theory, even when opposed by apparent facts, is contained in an article by Lord Rayleigh in the Philosophical Magazine for March 1889, " On the History of the Doctrine of Radiant Energy."

It appears that W. Herschel conceived the idea that the radiation which excites the sensation of light and the radiation which produces heat in black bodies are essentially different ; and this view, which was contrary to his original intuition, was based upon a crucial prismatic experiment, made for the purpose of ascertaining " whether the heat of the red rays is occasioned by the light of those rays " or not. A definite question, *answered by experiment in the negative.* On this Lord Rayleigh remarks :—

" I am disposed to think that it was this erroneous conclusion from experiment, more perhaps than preconceived views about caloric, that retarded progress in radiant heat for so many years. We are reminded of Darwin's saying that a bad observation is more mischievous than unsound theory. It would be interesting to inquire upon what grounds we now reject the plain answer which Herschel thought himself to have received from experiment. I do not recall a modern investigation in which the heat and light absorptions are proved to be equal for the various parts of the visible spectrum. Can it be that after all we have nothing but theory to oppose to Herschel's facts ?"

Yes, that is all, and, as Lord Rayleigh well knows, it is amply enough. Whoever examines the facts again will do

so not to substantiate our present theory but in the hope of up-setting it. Success is of course just conceivable, but, when it comes, there will be time enough to reopen the question. Lord Rayleigh's words may be distorted, and may even suggest false meanings to minds with a crooked turn in them ; so may many of the apparent admissions about unprovableness in this paper of mine, but, whether it gives occasion to the enemy to blaspheme or not, it is true that a host of doctrines are believed because they form part of a consistent scheme rather than because of any seriously attempted, still less any really achieved, experimental proof. And to pull one of these neatly fitting blocks from its niche will demand the strength of more than one, of more than several, so-called crucial experiments. There comes a time indeed when the weight of experimental evidence suffices to uproot a portion of tightly fitting theory ; but seldom, as I think, without some looseness or uneasiness being first detected, and never without a betrayal of rottenness at the root.

Why do Physicists deny that matter can be moved by mental power from a distance without physical mechanism ? Why does modern science reject the whole of a certain class of miracles, in the teeth of an immense record of direct experimental evidence ? Solely because these things do not fit in with such coherent views of the universe as they have at present been able to frame.

Why, again, do we accept a multitude of unverified state-ments, such as, that every portion of radiation, whether it be of light or of sound, is intrinsically energy, and must, if absorbed, result in heat ; that every muscular contraction of an animal corresponds to the combustion of a portion of his food ; that a given gas consists of particles of approximately specified size and weight travelling at a certain average speed; that a medium connects every pair of bodies which are perceived to exert force on each other ; and so on ? Solely because these things do fit into a coherent and self-consistent scheme of the universe.

Any scheme or doctrine *sufficiently* harmonious and consistent goes far thereby towards establishing itself as truth. So conspicuously is this the case, when one comes to reflect, that there are not wanting some who conjecture that by our

thought we are, so to speak, *constructing*, or at least helping to construct, the cosmic scheme.

Some axioms the human race has now given up challenging, and by so abstaining has silently accepted as corresponding to the truth of things. Others it occasionally exercises its ingenuity in degrading or depreciating, not into untruths, but into special cases of a higher and super-sensuous generalization. Varieties of space are imagined, and mathematically treated, where more than one line can be drawn through a point parallel to a given line, where the shortest distance between two points is not straight, where the three angles of a triangle are not equal to half a revolution, where a closed surface is an incomplete boundary, and where more than three lines can be perpendicular to each other. These things are imagined, and for all I know they may in some occult fashion exist. To set bounds to the possibilities of the universe on the limited evidence of our few sense-organs would be absurd. But I say that any proof of their actual existence within our more developed ken rests with the experimentalist. As soon as facts are forthcoming which clearly and definitely are inexplicable on the basis of our present notions concerning space, I for one am willing to enlarge those notions and to contemplate provisionally whatever hypothesis suggests itself as most simple and plausible. Till then there is plenty of work for a physicist in interpreting, systematizing, and clarifying the facts of the universe, as it appeals to him through the agency of his ordinary three-dimensional senses, aided by his undimensional common-sense.

But I hold that with all these vague possibilities of ultimate development in front of him (not so vague but that they are in some sort conceivable, or at least tractable by reason), a natural philosopher need not confuse himself by endeavouring to complicate what is already transparently simple ; nor will he be wise to attempt an over-laborious scrutiny of his fundamental axioms ; for the more neatly and quietly he can lay his foundations the more time will he have for building the superstructure, and the more gorgeous he may hope to make it. By all means let him avoid a rotten or insecure element in his foundation. It must be as sound and strong as possible ; but his underground work need not be decorated with fanciful

and laborious conceits, it may be as plain as it is substantial, and he may leave its edges rough in order to connection with structures as yet unbuilt and unimagined.

Of this plain and substantial character would I seek to keep the laws of motion. If a statement like the first law of motion cannot be made in simple and readily intelligible language, I should despair of Physics. In that case the Physics of the future could be little better than a barbarous jargon of technicalities. There are plenty of really difficult places where technical language and unfamiliar modes of thought are for the present essential. The developments of the super-structure erected during the present century are indeed now so stupendous that for myself I should be satisfied if, without appreciably adding to them, I could by consolidation and restatement remove the necessity for some of this artificiality, and so make their harmony and beauty more readily appre-ciated. But in order to do this a simple and unlaborious foun-dation is a necessity. I hope to try before long to display the bold outlines of the foundation already laid by men of sur-passing genius, every unnecessary accretion being cleared away, and the whole simplified to the uttermost ; and then, if the attempt be not too ambitious, I should wish to extend the process to some portion of the superstructure.

Part II.

The First and Third Laws of Motion.

So much for general preamble ; now turning to Dr. MacGregor's address, we find that he objects to the Newtonian statement of the first law of motion on the same ground that Prof. Karl Pearson, Prof. Mach, Mr. Macaulay of King's College, Cambridge, and several others* have objected to it, on the ground, namely, that uniform motion is unin-telligible or meaningless unless you specify its direction and velocity with reference to a set of axes. And directly you try to specify axes you get into difficulties, for, although a uniform translation is permissible to them, any rotation or any acceleration of the axes is fatal to a simple state-

* See for instance a correspondent in ' Nature,' vol. xxxvi. p. 366.

ment of the behaviour of a body acted on by no force. It is useless to say that the axes must be stationary, because one cannot define what that means ; so the attempt is made to say that the axes must not rotate and must not be acted on by force ; but this last condition is of no use unless they possess inertia; so the axes are sometimes supposed to be generated by particles of matter projected in different directions and subsequently free from force* ; or else the axes are made of infinite mass so that no finite force may be able to affect them.

Now I hold that all such notions as axes of reference are artificial scaffolding, necessary for the numerical specification of a velocity, but not at all necessary for the apprehension of what is meant by a uniform velocity. It is in the specification of any absolute velocity that the difficulties cited about an origin and axes of reference legitimately occur. It is in fact impossible to specify the absolute velocity of anything, because we have literally no criterion of rest. We shall, I believe, hereafter find it convenient to postulate the Ether as a body absolutely at rest; but none of these physical or geometrical complications should enter into an axiomatic statement.

All that the first law asserts is that the motion of a body not acted on by force is uniform in magnitude and direction. There is no need to attempt the impossible and say what that magnitude and direction absolutely are. Whatever they are they remain constant. If asked to prove this statement, we should at once decline, and throw the burden of disproof on the doubter. This is what Maxwell does† when he says (virtually) :—If the speed and direction of a freely moving body vary they must vary in some definite manner; very well, tell me in what manner they are varying. You cannot, unless you can show me absolutely fixed lines of reference.

The fact is that the conception of uniform motion is based upon a simple primary muscular sensation, or at any rate upon

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* Thomson and Tait, vol. i. Part I. (1879), § 249. But although these writers do propose to use such axes to fix direction, or, better, the invariable plane of a rotating system, §§ 267, 245, they quite logically deduce these things from Newton's laws, and do not use them in the statement of those aws.

† Matter and Motion, art. xli. p. 36.

a succession of such sensations; everybody understands what it means, so far as it is possible to understand anything in this material universe, and the sense in which it is understood is amply sufficient as a basis for a physical superstructure. The first law is a true axiom, and its boldest and simplest form is not only the best, but is the only one that can with any justice be called axiomatic. How can one appeal to the experience of the human race with reference to coordinate axes of infinite or any other mass? How can we utilize as axes the trajectory of particles free from force, without tacitly assuming the first law continually? The whole attempt to complicate the statement of the first law of motion seems to me absurd.

The well-known other objection, that a statement of the first law is unnecessary because it is only a special case of the second, rests on a different footing. Thomson and Tait have pointed out that it acts as a definition of equal intervals of time. Prof. MacGregor denies that the first law gives us any more useful definition of time than the second does; but seeing that every clock is an attempt at a uniform mover, and that the second law is concerned with the more complex notion of acceleration rather than with the simple idea of velocity, I do not imagine that he will seriously adhere to this view : and therefore, fully admitting the obvious fact that the first law is a special case of the second, I still hold that its separate statement is desirable, because it is so simple, and because it does afford a clear practical definition of the mode of measuring time. But were this its intended meaning it could have been expressed more straightforwardly. Newton probably considered it as a qualitative statement introductory to his second law : and as such it is entirely suitable.

None of the objectors to the first law have the slightest doubt of its truth,—that is what makes their contentions so practically futile ; it would appear that it is too simple to please them; they seem to wish to complicate its statement so as to make it look more like the difficult things with which they are accustomed to deal. I feel convinced that many mathematicians mistrust a simple statement in English, and hardly conceive that such a language can really express an important law ; their trained ability to deal with difficult conceptions leads them, as I think, astray.

I am not quite clear what the word logomachy means, but much of the discussion which has been bandied about concerning the statement of the first law seems to me to be rightly designated by some such opprobrious or perhaps complimentary epithet.

When Dr. MacGregor goes on to consider the third law, and to deny that it can properly be regarded as a deduction from the first, he is urging a very minor matter, if what I have said concerning axioms has any truth in it. Still the question has some interest. Whether it is deducible from the first law or not may be held to depend on how general the terms are in which that law has been stated. If it can be axiomatically asserted that the *centre of mass of a rigid system* moves uniformly until an external force acts on the system, and also that the system does not begin to spin, then the third law is established. For since zero acceleration means zero force, it follows that all the internal forces add up to zero, and have no moment; and since the system can be dissected bit by bit without ceasing to be a system within the scope of the first law, it follows that no stress can contain an unbalanced force or couple.

If the law be doubted for the case of a pair of bodies attracting each other from a distance, Newton says* (virtually):— Jam the bodies apart with a rigid obstacle, then you have reduced their action to contact action ; and since you have a balanced stress at each point of contact, and likewise between the ends of the introduced obstacle, it follows that the attracting forces of the distant bodies are also balanced.

Now Dr. MacGregor's objections are (1) that Newton's proof only aims at extending the law from contact action to actions across a distance, while for contact action he is content to assume it as an axiom or to verify it by experiment ; and (2) that the proof breaks down for a particle or single body which cannot be analysed into parts.

It is quite possible that Newton thought it best to state his third law as an axiom ; because the fact that the centre of mass of a complex system of bodies obeys the first law is hardly an experience that can be confidently appealed to, even

* *Principia, Scholium* to *Axiomata*.

though all the bodies are in contact. That fact and the third law are intimately *connected*, but whichever is the simpler had better be stated as the axiom, and the other be made a deduction from it. Thus the centre of mass statement follows from the third law, and so very likely Newton preferred to arrange it. It is frequently difficult to know which of two very simple statements is the more axiomatic; and methods of proof are notoriously susceptible of considerable variation. The important thing is to notice the link or tie between two facts, to show that they mutually strengthen each other, not to pretend that one is beneath the other and supports it.

Attempts to build even so simple a structure as geometry in the form of a single column, stone upon stone, have been found artificial and in the long run impracticable. An enlarged basis of direct appeal to experience is not only necessary but desirable, and all fundamental matters should be kept low down, as nearly in contact with first principles as possible.

The so-called deduction of the third law from the first or second is important as a clear and strong cross-connexion between the two things, and need not be considered as a rigorous proof. It is rigorous enough if the premises are granted, but if not, then there is a certain outstanding axiomatic or unprovable character to be shared between them; but this outstanding portion is, by reason of the cross-connexion, so slight as to remove all difficulty from what otherwise would seem, and indeed, strange to say, to many still does seem, an exceedingly tough morsel to swallow.

[I mean that one constantly finds examination candidates, and even Engineers, when catechized about a horse pulling a cart, though they may, some of them, politely admit that approximately the pull of the horse and the pull of the cart are equal (constituting the stress in the trace whose inertia we may agree to neglect), yet nevertheless assert that in reality the pull of the horse must be *the least little bit* bigger than the pull back of the trace, else the thing could not start. The fact is that the universal truth of the third law is *not* axiomatic, or at least is not obvious*, and hence its deduction from the other two laws is really a useful deduction.]

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* See for instance 'Nature,' vol. xxxvii. p. 558; and see all recent volumes of 'The Engineer' *passim*, especially about 1885 and 1891.

I do not see any point in Dr. MacGregor's second objection that the proof is inapplicable to a body without parts. For if such a body anywhere exists, plainly its parts cannot act on each other, and so there are no actions or reactions in such a body worth troubling about.

The concluding portion of Prof. MacGregor's address has to do with the Conservation of Energy and the question of how far it can be deduced from the third law. But all these questions respecting energy we must fight out at greater length. Dr. MacGregor well knows that from the third law and the denial of action at distance together I claim to have deduced a law of conservation simpler and more precise than the ordinary law; but his objection to this* is that though such a law may come to be accepted as sufficient in the future, when the universality of contact-action is fully recognized, it is inadequate for the present, when action at a distance still holds a portion of the field ; by which I suppose he really only means that many mathematical methods of treatment are based at present on action-at-a-distance modes of expression. I have no fault to find with any convenient mode of attacking specific problems ; it is permissible to everyone to use the language of distance-action for practical purposes ; but when it comes to formulating fundamental laws I have no ambition to legislate for such cases until they can be shown actually to occur. I am open to experimental proof of their existence, but to none other. It is premature to legislate for them. If any action other than contact-action exists, we had better know more about it before formulating its laws. If the will-power of a "medium" for instance can really move a chair without any kind of contact or physical connexion, it will be wise to look carefully for the seat of the other component of the stress, if there is one, and for the source of the energy concerned, but I myself should feel extremely hazy as to their probable locality.

When action at a distance does present itself in Nature (and if it ever does it is clearly going to be in connexion with the operations of Life), it will be very well to overhaul our axioms to see if they require modification. Till then I propose to state them in terms of the facts we know. This I will attempt in another Part.

* Expressed elsewhere, viz. Phil. Mag. February 1893.

Part III.

The Conservation of Energy and Universal Contact-action.

The ground is now clear, I think, for a reply to Professor MacGregor's criticism, as made in the Phil. Mag. (vol. xxxv. p. 134) for February 1893 ; and incidentally I may hope to answer or at least discuss the matter with some other critics, notably Mr. Heaviside in his paper in the Phil. Trans. 1892.

The first objection is that in my definition of energy I assume the ordinary law of conservation, because I say, in an early paper (Phil. Mag. Oct. 1879, viii. p. 278), "Whenever work is done upon a body, an effect is produced in it which is found to increase the working-power of that body (by an amount not greater than the work done) ; hence this effect is called energy, and it is measured by the quantity of work done in producing it." "The words ' is found,' " says Dr. MacGregor, "indicate an appeal to experience." Most true, so they do. My position is this :—Before making any definition it is desirable and only civil to show the reasonableness of it. To thrust a statement out without preamble or explanation, under cover of the contention that being only a definition or an enunciation one is at liberty to define or enunciate as one pleases, is I fear a thing frequently done, but it is barely polite, and it is apt to excite either resentment or else undue and slavish submission.

It is from no lack of love for Cambridge and the great men she has nurtured that I venture to hold that the typically Cambridge plan of text-book is liable to err in this direction. An attitude of blind faith and mere assimilation is required of a student for many of the earlier chapters, sometimes for the whole of a book. The life, the interest, of the subject to be treated, an exhibition of the reasonableness of the adopted mode of treating it, are all neglected, and a ghastly skeleton is presented to you bone by bone ; which truly is an admirable structure when subsequently clothed in flesh by men of sufficient genius, but which is liable to excite repulsion in a

thinking student who possesses some physical instincts but is
devoid of the artificial galvanic stimulus called Examination*.

It may be that I myself err in an opposite direction, being
so interested in the muscles and the clothing that I forget
here and there a bone or two. If it be so it is a grievous
fault, and grievously shall I have to answer for it. It is not
a fault that I ever attempt to justify. Flabby and boneless
science is no science at all.

In defining Energy, then, I first appeal to experience that
something in the popular sense *energetic* is often plainly pro-
duced in a body when work is done upon it ; *e. g.* when a bow
is stretched or a stone flung ; and I proceed to say that that
something is always produced, even when not obvious, and
that it has been called Energy because it frequently confers
upon the body possessing it the power of itself doing work.
I know well enough that the common definition runs, Energy
= power of doing work, but there is a difficulty about this
definition : plenty of energy has no power of doing work, or
at least no power that we can get hold of. Therefore I pre-
fer to give a name to the result of work done, whether it be
obviously energetic or not, and to justify the name energy by
appealing to the many cases where its possession does confer
a power of doing at any rate some work ; but in the definition
I make no statement that energy must necessarily be able to
do work, or must necessarily continue constant in quantity,
because such statements, so far as they are true, are part of
the law which is being led up to, not part of the definition.

My definition of "energy" stands on all fours with the
customary definition of the potential function. It is a name
for the line-integral of a force, considered as a quantity that
can be stored. The line-integral of a force in action is *work*,
the result of it is *energy*. Or, otherwise :—The scalar product
of force and velocity is *activity* (sometimes called power) ; the
result of activity lasting a finite time is energy.

Some conception of what energy is like can be gained by
appeals to familiar experience ; and the fact that it can be

* Valuable enough, however, in its proper place. I am not joining in the
outcry against examinations. They are a most useful and much needed
stimulus to right intellectual conduct, and partially replace the old belief
in purgatory.

stored more or less completely, and can ultimately reproduce
an activity similar to that which generated it, can also be
illustrated in a selection of cases. The law of energy asserts,
what cannot be so readily demonstrated, that in *all* cases it
can be stored without loss, except in so far as it leaks away,
or until it is discharged, in some kind of equivalent activity ;
and that even when thus dispersed or transferred or lost to
the body in which it was generated, it still exists in other
bodies in undiminished quantity, and so is capable of trans-
ference and retransference, by means of activity, and activity
alone, for ever.

But how is such a wide-sweeping statement to be justified ?
Not by any hasty appeals to experience, not even by the
laborious researches of a Joule. Such experiments can indeed
examine the obviously weak places and can show that there
is no manifest flaw there, that all the apparent flaws dis-
appear on closer inspection, and, so far as can be seen, do
not exist. The law itself as a universal generalization must
be itself axiomatic, or else must be deduced from some other
and simpler axioms.

I hold that it can be deduced from Newton's third law and
from the denial of action at a distance, thus :—Bodies can
only act on one another while in contact, hence if they move
they must move over the same distance * ; but their action con-
sists of a pair of equal opposite forces ; therefore the works they
do, or their activities, are equal and opposite ; therefore, by
definition (p. 303), whatever energy the one loses the other gains.
In other words : in all cases of activity, energy is simply trans-
ferred from one body to another, without alteration in quantity.

I claim also that the law of conservation thus established is
more precise than the ordinary law (which I confess always
seems to me rather vague, especially when such absurdities

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* Motion in the line of stress is here directly contemplated. As has
been already several times noticed, the possible *slipping* of bodies in con-
tact is not apparently allowed for, but in so far as they are smooth their
slip is ineffective, and if they are rough there is a tangential stress to be
taken into account as well as a normal pressure ; the friction-stress is
intermittent instead of steady, and molecular instead of molar, but other-
wise the above statement applies without complication, so far as it
ever applies to the immediate action of ordinary material bodies.

as "possible" and "actual" energy are put into its state-
ment,—a denier of the law could use no more deadly word
than "possible"); more precise and definite, I say, because it
is the law not only of *conservation* but of *identity*.   I believe
that questions arising from this law of the identity of energy—
a study of the paths by which any given now-existing bit of
energy has reached its present locality, and of all that has
happened to it in the past—may prove in the future to be as
fruitful a region of enquiry as are studies in the history of any
given piece of matter, say the earth or the sun, or, to step
forward a little, the history of an individual mind, when we
realise some day that this, too, has a continuous, and perhaps
traceable, existence.

However, Prof. MacGregor objects (p. 135) that my deduc-
tion of the conservation of energy proves it during *transfer*
merely, and leaves untouched the question of what happens
during simple *storage*.

That is true, and I do not think that I had noticed the
objection before. My deduction proves that in all cases of
activity energy passes on without loss ; it does not prove that
without any activity energy may not leak away in some silent
unobtrusive fashion, leaving no trace, but simply vanishing.

To meet this objection, an appeal may be made to the
definition ; it may be called on to assert that anything that
can thus disappear without force or motion is not energy,
according to its terms; but I doubt whether such an appeal
is perfectly cogent, though it has some, perhaps much, plausi-
bility.   I think that the non-disappearance of energy in this
occult manner had perhaps better be regarded as an axiom, a
statement which may be believed until some clear experi-
mental disproof is forthcoming : no such disproof being
meanwhile in the least expected.

Briefly summarised, the matter stands thus :—

(1) If energy can only be got rid of by activity, and (2) if
that activity never affects its quantity, the law of conservation
is completely stated.   My proof covers (2), but not (1).   I
may therefore agree with what I suppose Prof. MacGregor to
mean, that the portion (1) must be left as an axiom based on
experience.

Prof. MacGregor goes on to say (p. 137) that my law of

conservation during mutual action has no deeper meaning than the conservation of their joint momentum, and is quite consistent with the non-conservation of their working-power.

To this I reply, first, that I mistrust the vague term " working-power," it is apt to mean whatever may be convenient ; from one point of view a given amount of energy may have an infinite " working-power," since it can do work at every transfer without itself diminishing ; while from another point of view it is rather bold to maintain the conservation of working-power in face of the doctrine of the dissipation of energy. And secondly I reply, that the conservation of *momentum* rests on the equality of the forces exerted by two mutually operative bodies, combined with the obvious equality of the *durations* or times of action of these forces. Whereas the conservation of *energy* rests on the equality of the forces, combined with the altogether less obvious fact of the equality of *velocities*, or *distances traversed,* by each of two operative bodies. Anyone in his senses who believes in action at a distance would deny that the velocities of two directly acting bodies are necessarily equal (no other bodies being assumed present, so as to simplify the problem) ; and in so doing he practically denies the conservation of energy.

Energy is only really conserved under conditions of universal contact-action.

Of course the believer in action at a distance is not at a loss : in order to retain his fiction he has invented another unreality, which he calls possible energy. He says (truly enough for many purposes) that when a stone is raised from the earth a great deal more work is done on the stone than on the earth ; and hence that, although at every instant the stone and earth have equal opposite momenta, the energy is nearly all possessed by the raised stone. But since the energy of an inert mass is by no means apparent, since it only has the power of gaining actual energy when let drop, its energy when inert and merely elevated is called possible or potential. Or sometimes, rather more accurately but much more vaguely, energy is said to belong to the *configuration* of earth and stone *.

Now this idea of potential energy is convenient as a

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* *Cf.* Phil. Mag. June 1881, p. 532, and June 1885, xix. p. 484.

mathematical expression, and used in its proper signification it is an essential reality ; but used in the sense above quoted, a very common mode of using it, it is a mere receptacle for stowing away any portion of energy which it is not convenient for the moment to attend to ; and I defy anyone to realize it as a thing.

Yet I myself constantly employ the term potential or static energy, and assert that every activity not only transfers energy from one body to another, but also transforms it from kinetic to potential, or *vice versâ*.

Yes, but I also assert that the transformation can only accompany transference, and that transference cannot occur without transformation. Whereas, on the ordinary view, the energy of the raised stone is supposed to gradually transform itself into kinetic as the stone drops, but to remain in the stone all the time.

I say that the energy was no more in the stone when merely elevated than it is in a strung arrow or the bullet of a cartridge. The energy is in the bow, or in the powder, and is rightly said to be in a static, or strain, or potential form. It can transfer itself to the projectile, and simultaneously transform itself into kinetic, at the pull of a trigger *.

It is easy to express oneself so as to be understood to object to the whole idea of potential energy. It is not to the use of the term that I object, nor even to its misuse, so long as it be done in the temporary interest of some specific problem. It is by no means necessary always to attend to everything : acting mechanism is frequently, with convenience and brevity, ignored, especially when one is really ignorant of its dynamical nature ; but in laying foundations we should not make these omissions and slurrings. It is the erroneous localization of energy in a fundamental or theoretical

* On page 141, Prof. MacGregor makes a statement in which I entirely fail to catch his meaning. It runs as follows :—" When Prof. Lodge states that ' a bullet fired upwards gradually transfers its undissipated energy to the gravitation medium, transforming it at the same time into potential,' he seems to me to assume that the bullet is rigid and that the medium is without inertia." Surely my critic does not consider that either plasticity in a projectile or inertia in a gravitating medium is essentially involved in determining the height attained by a body thrown up.

treatment of the subject that I deprecate, and I believe that everyone will agree that in all such treatment convenient fictions are better avoided.

There certainly exists potential energy in the case of a raised stone, but it does not belong to the stone ; it belongs to the medium, whatever it is, which is exerting force on the stone and on the earth, and pressing the two lumps of matter together. It is properly called static, as distinguished from kinetic, because it is the energy of force, not the energy of motion *. As I have frequently pointed out, the two forms of energy correspond to the two factors of the product work or activity. Both factors are necessary for the actual performance of work. Until both factors are present, the energy s merely stored. As soon as the missing factor is supplied, it is *transferred* from the body acting to the body acted upon, *e. g.*, from the gravitation medium to the stone.

But it will be said, suppose the stone is not allowed to drop freely, but is used as a clock-weight ; it is doing work all the time it slowly falls, hence when raised it must have had energy. But in this case I should deny that the stone is doing anything active ; it is a necessary concomitant, it is a link of communication, it is not really itself doing work. Its presence indeed and that of the earth are necessary to the existence of the stress in the ether [permitting that or some other equivalent hypothesis for the moment] ; but it is the ether stress which is doing the work and driving the wheels of the clock. A clock-weight has somewhat the same function as the string of a bow ; it was the convenient means of generating the potential energy, and it is the means of communicating it to the

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* I do not mean to assert or deny anything concerning the nature of gravity. It may be that all energy is *ultimately* of the nature of motion, as we know Lord Kelvin has taught us may be the case with elastic stress for instance ; but *force* is the proximate mode of action of a coiled spring, and so it may properly be described as static energy, whatever its ultimate nature. The reasons for affixing to static energy the epithet potential are mainly mathematical and historical. There is much convenience in thus linking it on to the potential function, and there is no particular inconvenience unless the adjective be misunderstood in the sense of " possible." It is indeed possible *work* or possible *activity*, but so is every kind of available energy ; it is not possible *energy*, for that term is equivalent to the assertion that not yet is it energy at all.

thing to be driven, but it is not itself energetic, any more than the chain and ratchet-wheel which helped to wind it up were energetic. Or, to take another example, it is like the piston of an air-gun : without it the gas would not have been compressed, and it might be (though it is not usually) employed to let the pressure reversibly down again, but it is not itself an active agent either in the charge or the discharge. It is like a Holtz machine or a dynamo : needful to the charging of Leyden jars or storage-cells, but not in itself doing the work, either when used for charging or, when running as a motor, for discharging.

I fear I am explaining elementary matters at some length ; but, although elementary in one sense, they are not quite easy to seize by the right end ; and the fact that they are so elementary tends actually to retard their apprehension, for unless people have patience to think them out they will not grasp what is meant. The difficulty in all these matters is that everybody thinks he understands them already, and is quite satisfied with his own prehistoric way of regarding them. Now I want to say that I have thought these things out with some care and labour ; and I believe that although no doubt everybody does understand them sufficiently for practical purposes, yet, if anyone has faith and patience enough to consent to reconsider them from my point of view (assuming, of course, as in most cases I safely may, that it is not his already), he will sooner or later realize the advantage of it. This belief may be presumptuous, but if so I am willing to presume to that extent.

With the substance of the following quotation from Professor MacGregor, on p. 138, February Phil. Mag., I entirely agree.

" If there be actions in nature which are not actions at constant distance " [i. e. practically contact-actions] " Prof. Lodge's law is not applicable to them, while the ordinary law is. Even if it be admitted that all actions in nature are contact-actions, there are many groups of phenomena which, in the present state of our knowledge of them, cannot be investigated on the hypothesis of contact-action. The early stages of their investigation must be conducted by the aid of the fiction of action at a distance ; and in such stages Prof.

Lodge's law is not applicable, while the ordinary law is. Hence Prof. Lodge's law is not so general in its applicability as the ordinary law."

I might prefer to express it differently : I would say that it is no discredit to the true law not to lend itself to fictions ; but I would also say, and freely admit, that it is often permissible for practical purposes to work in a fictitious or incomplete manner (as, for instance, when dealing with the diurnal motion of the sun or the libration of the moon), ignoring communicating mechanism in cases where one is ignorant of or need not attend to its nature, and in fact proceeding as the great physicists have constantly proceeded when engaged on a practical quest.

Never have I imagined that the practice of men of genius is other than the practice most suited to their immediate end. But when it comes to the philosophy and essence of the matter, I do think that there is some fear lest a constant habit of ignoring unknown essentials for practical and temporary convenience should lead these great mathematicians into forgetting that they have ignored anything, should in the long run delude them into treating their fictions as if they corresponded to the reality of things ; and it is permissible for me or others, as lookers-on seeing the structure from another point of view, unobtrusively to point out to the master builders, in their moments of leisure, the fact that while they have been engaged in the upper stories the foundations have almost automatically expanded, that the available basis of axiomatic reality has become broader and simpler than could be perceived when they started on their work, and that now some portion of their temporary scaffolding and underpinning may without danger, and with advantage to the general appreciation of the fabric, be discreetly and quietly removed.

PART IV.

The Dissipation of Energy, the Nature of Potential Energy, and the Second Law of Thermodynamics.

Passing on to Prof. MacGregor's fourth objection (p. 140) that I have not proved that transformation and transference necessarily go together, I hope to be able to meet him more

nearly than has been possible to me on most of the other counts of his indictment.

My proposition was that the change of form is always from kinetic to potential or *vice versâ*. But it is not necessary to attach the same importance to this law as an ultimate fact, because it is so extremely probable that most or all cases of potential energy will ultimately be resolved into essential motion. Nevertheless it is for the present a convenient statement of truth, having the same sort of validity that the second law of Thermodynamics possesses.

The following is my demonstration of the law of transference and transformation : being indeed the detailed statement of the essential phenomena accompanying transfer of energy, on the hypothesis of contact-action.

A body * or other medium exerting force at any point, and there moving in the sense of the force it is exerting, is in a state of activity : it is passing energy on ahead. If its speed continues constant, the force just spoken of cannot be the sole force, the resultant force on it must be zero ; in that case it is a mere transmitter, not itself active, only passing on what it receives. But if it is itself active, i. e. parting with its own energy, then its speed at the acting point (the place of application of the force) must either decrease or increase.

If its speed decreases, it must be parting with kinetic energy ; if its speed increases, it is parting with potential energy.

Contrariwise :—

A body or other medium exerting force and being moved in the sense opposed to that force is receiving energy.

If it is not merely passing it on, in which case the resultant force acting on it is zero, its velocity must vary.

If the velocity increases it is gaining kinetic energy ; if its velocity decreases it is gaining potential.

Now the two bodies or things here spoken of are necessarily existent in every case of activity ; one is the agent the other the patient, one the emitter the other the receiver, one

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* Meaning by " a body " a thing of constant mass, an identical lump of matter. A thing of variable mass, like a rain-drop or a railway-train or a layer of moving gas, is gaining or losing matter as well as energy ; and cannot be regarded as one simple *body*.

the acter the other the reacter, one the driver the other the driven ; and they are in contact while the activity lasts

Being in contact, their velocities along the line of stress must increase or decrease together.

If their common velocity is decreasing, then the driver loses kinetic and the driven gains potential.

If their common velocity is increasing, the driver loses potential, the driven gains kinetic.

This is my proof of the necessary concomitance of transfer and transformation, and of the alternation of transformations. But it will be well to illustrate the matter further.

Let me first explain how I define potential or static energy as used in the proposition criticised. I mean by it simply the energy of a body under stress ; an elastic body which is exerting force may always be said to have potential energy, notwithstanding that when the force comes to be analysed it may be perceived to result from some kind of motion. Thus a reservoir of compressed air is a store of potential energy, though it is well known that all the pressure is due to the motion of unstressed particles, and is really just as much kinetic as any of the energy of planets and comets. A powder-magazine is a store of potential energy, though quite possibly the forces awaiting a trigger to liberate them into activity are due to atomic or etherial motions ; and the same may be said of a charged Leyden jar and of a bent bow.

Briefly the idea of potential energy corresponds to the idea of elastic force when the cause of that force is not sought. The idea of kinetic energy corresponds to the idea of sustained motion. Each corresponds to one of the factors in the product "activity," or $\mathbf{F}v$.

In order that a body may possess energy it must be *capable* of exerting force and also of moving ; but it need be *doing* only one of these things, and so long as it merely stores energy it *must* be doing only one of them. Both factors must concur before it can be active, both factors must be possible before it can be said to possess energy. The only question is, which factor does it possess meanwhile ? If it is exerting force, but stationary, then its energy is potential or static ; if it is moving quite freely, then its energy is kinetic

So long as the other factor is absent no activity manifests itself, no work is done, and the energy is merely stored. Directly the other factor is supplied transference begins, energy leaves the body storing it, and passes to the body supplying the other factor.

Now to make a *moving* body do work, a resisting obstacle must be supplied, and this second body thus exerting force receives a due proportion of the energy; and it receives it by reason of the force it exerts, that is, it receives static or potential energy from a body which possessed kinetic.

To make a *strained* body do work, motion must be permitted; the thing which is moved gains energy, and it gains it in the kinetic form from a body possessing potential.

Of course in both cases the body immediately receiving the energy need not retain it, but may rapidly or instantaneously pass it on to something else—in which case it resumes its first form; by simple alternation of transformations.

Also it is not essential that the potential energy of the particles of a spring shall be communicated to any *foreign* body: if it is transferred to the spring as a whole, considered as a mass possessing inertia, that is sufficient; in other words, it may be transferred from one set of particles to another set of particles, all forming part of what we usually speak of as a single body *.

To take one of the examples of Prof. MacGregor, viz. a loaded air-gun with its muzzle plugged, so that its bullet or wad is not allowed to be shot. The compressed air has potential energy; on its release its energy is transferred to the moving wad, which instantaneously hands it on to the air near the muzzle, compressing it, and thus retransforming itself into the potential form.

If it be objected that the energy never at any instant all existed in the kinetic form, the answer is no, but it necessarily passed through that form; it could only be by the intervention of motion that the energy of the air at one end was transferred to the air at the other end. The wad itself is unnecessary and may be dematerialized; the only thing moving

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* This sentence is true in one sense, but it is not a final or complete statement, and in a later paper a modification will be introduced.

may be the gas atoms themselves, but the atoms which are rushing are not the same as those under strain; the moving atoms are precisely those which have escaped from strain. As everybody knows, where the rush of gas is greatest in a constricted pipe there the pressure is least. Air rushing from one reservoir into another illustrates the law precisely, though I choose it as a case at first sight favourable to Prof. MacGregor's contention; in the narrow part of the pipe air is flowing against a higher pressure in front of it, it is compressing the air ahead of it by reason of its own momentum; the energy of each molecule is availably kinetic to precisely the extent to which it ceases to be potential, and any residual potential energy which does not become momentarily kinetic, and kinetic in the available sense of a windy rush, has not been transferred at all, but remains simply stored, as it would have been had nothing been released and no activity been manifested.

If the two reservoirs (or two Leyden jars) are for instance equal in capacity, and if one had been originally empty, then one quarter the original energy remains still potential in the originally full vessel, another quarter has been transferred by the rush of gas into the originally empty vessel, and two quarters have been dissipated, so to speak, converted into heat by friction. If there is no friction, then this half of the original energy remains alternating for ever from the kinetic to the potential form, like the bob of a pendulum, and transferring itself at every half swing from the one vessel to the other.

Lest this last statement should permit misconception, it may be necessary to state explicitly that the energy is only hazily attributed to the vessel—the energy belongs of course to the gas molecules—and, further, that the molecules which are in motion are not the same as those which are under strain. The energy, that is the portion of energy concerned with the particular amount of activity contemplated, is continuously leaving the molecules recovering from strain and transferring itself to those in accelerated motion, and as continuously leaving those in retarded motion and transferring itself to those acquiring strain*.

* See, however, the footnote on preceding page.

Further to avoid misconception it is necessary to admit distinctly that in speaking of the pressure-energy of a mass of gas we are subjecting ourselves to an unnecessary though convenient limitation. It is chosen as an example just *because* so much is known about it; it would have been easy to choose elastic solid examples, where the elastic stress is not so readily analysable, and where the idea of potential energy and the statistical grouping of a mass of molecules was not optional but necessary, in our present state of ignorance. I know well enough that the pressure of gas in a reservoir is really due to the motion of the particles, and I am willing freely to contemplate the case of Davy's "repulsive motion," no real contact or elastic impact between particles ever occurring, but only a rapid swing or asymptotic orbit round their common centre of gravity; but this rapid centripetal swing of particles getting within each other's molecular range is at present essentially an unknown process, and has to be relegated to the sphere of potential energy quite as much as in the not perhaps really dissimilar cases of elastic impact and gravitation.

Whenever an atom after collision retains its energy intact, there is no need to say that there has been either transference or transformation; mere retention or storage is sufficient for practical purposes (though if we attend to the details of a collision we find there really is always a double transference and a double transformation *); and if this is typical of what happens in every atomic encounter, whether it be with the

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* *E. g.*, suppose a quick molecule A strikes or otherwise collides with a slow but similar molecule B. The mechanism of the collision, being unknown, is unimportant, and is most conveniently thought of in terms of elasticity. A and B undergo distortion as they approach, and a certain portion of A's energy is communicated to whatever medium it is which keeps them asunder. This medium by its recoil then drives them apart, sending off B with the energy which A originally had, and transferring to A the small portion received from B. [This matter is discussed at greater length in Appendix 2.] Any part of the energy which was not transformed is also not transferred. Everything transferred from A to B necessarily underwent rapidly a double transformation. When other occurrences are analysed it will be found that the above is typical of what constantly happens. Yet it is not always necessary to attend to all details, and in the text above I speak as if A and B passed through each other, each retaining its original energy.

walls of the vessel or with another free molecule, then, so long
as the air remains compressed, there is merely a storage of
energy, each molecule possessing and retaining its own share.
But directly an opening is made, or a communication established
with an empty vessel, it is equivalent to the removal of a
portion of wall, and some of the atoms, with their energy, pass
along without obstruction into a new region of space.

In one sense each may still be said to retain its original
energy; but in order to regard it thus we must be capable of
dealing with the atoms individually. If the atoms escaping
from confinement are lost in the vastness of space, their energy
is unavailable, and their pressure, on which entirely depends
the potential energy of a compressed mass of gas, is *nil*. If
they escape into another equal vessel, then only the first
individuals can enter freely without obstruction, all the others
experience encounters on the way, as if the hole in the wall
were gradually being restored; and though at the end of the
operation, when everything has finally settled down, each
atom may be said to possess its original energy, yet the
pressure, on which depends the potential energy of the com-
pressed gas, has been halved, and the other half is unavailable
or has been dissipated.

Plainly the idea of potential energy belongs to the tempo-
rary order of ideas, to which the dissipation of energy and
the second law of thermodynamics belong, and is appropriate
to the present period when we have not yet learnt how to
deal with molecules individually.

The kinetic energy of a set of gas molecules is only
really available when it is combined with momentum or
angular momentum, *i. e.* when the motion of a majority of
molecules has the same sign, when there are more plus terms
than minus in either their translational or their angular
velocity; in other words, when there is a wind or cyclone *.

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* It may be worth while to point out that the reason why momentum
is necessarily preserved in cases of impact, while translatory energy is
usually not conserved, is because momentum depends on the first power
of velocity, and therefore is unconcerned with vibrations; while every kind
of vibration, whether it be of sound or of heat, enters into the sum total
of a thing depending on an even power of velocity, like energy, whose
translational value is therefore to that extent diminished by the pro-
duction of vibratory disturbances.

A stream of particles can be utilised, as by a windmill or a Pelton water-wheel, and that is an example of available kinetic energy. But usually the energy of fluids can only be utilised in the potential form ; as in a hydraulic-press or steam-boiler.

To say that all potential energy will turn out to be really kinetic may be true enough, but it is not at present a specially helpful truth, for even in cases (such as compressed air) when we actually *know* it to be kinetic, we are bound for all practical purposes to treat it as potential, since the available working-power of a spring, or of compressed air, of gunpowder, of Leyden jars, and of many other things, depends on their potential energy alone ; and such other energy as they may possess is conveniently ignored, as not concerned in any practical transformations or activities which we can bring about : it remains as an untransferable, and therefore inactive or useless residuum.

This is the real meaning of available energy, it is the portion concerned in transferences, the portion which can be transformed, *the portion which is able to transfer itself.* Potential energy, as commonly spoken of, is always of this character. An unwound watch-spring has lost its potential energy, it retains a quantity of untransferable heat-energy. A hot body cooling is in the same case ; so long as it is hot, some of its heat-energy is transferable ; and the transferable portion may well be regarded as and styled potential.

Available or potential heat-energy readily transfers itself to space or to other bodies, just as compressed air readily leaks out of its reservoir ; and in so doing becomes less potential*. Even if it is all received by other bodies in the room, the potential energy of the contents of the room is to the extent of the leakage diminished.

To say that heat-energy is constant in quantity is the same as saying that the molecules of an escaping gas need not part with their energy but may individually retain it. In the abstract either proposition is true, but just as it was only the compression part of gas energy that was potential or

* It is tempting thus to use the adjective " potential " in the sense of available. I do not at present wish to justify this secondary usage of the word, but I let it stand as a suggestive and harmless eccentricity.

available for us, so also it is only the high-temperature part of heat-energy that is similarly available.

It is customary and correct to say that the available part of heat-energy is converted into other forms when allowed to be active and do work. A distinction is thus apparently drawn between heat-engines, on the one hand, and water-engines, compressed-air engines, or electric engines, on the other; because in them the water, the air, and the electricity flow away undiminished in quantity and only at a lower level, pressure, or potential.

This distinction is the cause of some confusion or bewilderment, and I verily believe of some incredulity. Prof. Osborne Reynolds has shown, in his interesting biography of Joule, how great were the difficulties felt by the most eminent men in realising that heat actually disappeared in a heat-engine : that less heat was given to condenser than was received from boiler ; that not only temperature fell but *also* heat was lost ; and we all know how Carnot's theory was based on the contrary hypothesis.

But one mode of avoiding what is reasonably felt to be an artificial and puzzling distinction, is to say that in *all* the engines mentioned transformation of available energy occurs as soon as activity begins, and that that engine is perfect which enables all the energy available to transform itself in the desired way.

The potential energy of the raised water (so to speak for the moment), of the compressed air, of the separated chemicals, disappears, and transforms itself into the motion of a turbine, a bullet, or a motor-dynamo ; the potential energy of the hot " working-substance " disappears in like manner, and results in the motion of fly-wheel belts and shafting*.

It is inconvenient to speak of the energy of heat as kinetic. It *is* much of it kinetic, just as the energy of compressed air and everything else may be kinetic; but to us here and now its available portion is not kinetic, but potential. When we can

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* Prof. Fitzgerald has pointed out to me that this analogy will work well if I use *entropy* as the analogue of the water in a waterfall, because entropy does really fall in temperature and remain constant in quantity while a perfect heat-engine is working. This suggestion I hope to develop along with other thermodynamic matters in a future paper.

deal with molecules it can be regarded how we please—
alternately kinetic and potential, probably, like the energy of
a vibrating fork, or on the average half-and-half, like waves ;—
but till then the kinetic energy of individual atoms is useless, it
is the average energy of a group which alone is useful, and
this is all that we attempt to utilise in every one of the cases
cited. Practically useful kinetic molecular energy exists only
when all the molecules are rushing one way, and that is
never the case with heat.

By the energy of a spring we mean its energy over and
above its useless energy of average temperature; by the energy
of a storage-battery we mean the portion corresponding to the
reduced lead and the peroxide ; by the energy of a water-
fall we do not mean to include the warmth of the water above
absolute zero ; we always mean that portion of its total energy
which we aim at utilising, and so speaking we say that a
watch, or a dynamo, or a turbine, are efficient machines. By
the energy of a hot body we ought to mean that portion of
its energy which it has over and above its useless energy of
average temperature, the portion which it is willing and able to
part with, the only portion we hope to use or aim at using ; and
so speaking we might call a heat-engine an efficient machine.

We get a notion of low efficiency in the one case, of high
efficiency in the others—we confuse ourselves sometimes with
statements about the dissipation of energy—all because we
perversely *attend* to the energy of average temperature in the
one case but not in any of the others. We have done it
naturally enough, because in that one case our attention was
specially directed to the subject of heat ; but it may be a help
to realise that all the cases are essentially similar and on the
same footing.

True a steam-engine and boiler is *not* an efficient ar-
rangement, only about 8 or 9 per cent. at the best, but that
is because of the great unnecessary drop of temperature
between furnace and boiler ; starting with the temperature
of the *boiler*, and ignoring all energy below the temperature
of the condenser, it may be efficient enough, 80 or 90 per
cent. I suppose. It is better not to pretend to be able to
use average molecular energy until we have learnt how to
do it.

The portion of neat which can at a single operation be converted into work is very nearly the same as that which leaks away when the body is allowed to cool ; just as it is the potential portion which disappears from a standing Leyden jar, or a running-down weight, or a rusting spring, or a leaky reservoir. The difference between the cases is that whereas the capacity of Leyden jars and tanks is constant, capacity for heat is apt to vary with the other conditions of a body; hence intrinsic energy is not solely a function of temperature, but subordinately of volume also. This fact necessitates caution before the above statement can be regarded as complete.

What a body will freely yield throughout a cycle of operations, that can be utilised. We cannot advantageously gain energy by pumping it. Whatever must be pumped is unavailable. We require an artesian well or automatic supply of energy if we are to get work out of it.

A further statement also is necessary if we are to concern ourselves with practical modes of utilising the theoretically available energy ; some such statement as the following :— Available energy can only be continually utilised by means of reversible operations. Unless the working-substance is restored to its original condition the process cannot continually go on ; and any operation which is not reversible involves dissipation of energy or needless waste of availability.

That the available portion of heat should bear to the whole heat the same ratio which the available drop of temperature bears to the absolute temperature, is essentially but a definition of temperature ; it is an assertion that temperature is best measured as proportional to heat, or that the zero of temperature may conveniently be taken to correspond with the zero of heat.

An essential part of the second law of Thermodynamics therefore proceeds to state itself in a very general and purely commonsense form, thus :—

The portion of energy which a body can automatically part with is alone available for doing work; and only that portion which is parted with reversibly is actually utilised.

There is no need to mention "heat;" it is equally true of every form of energy. When a cell has run down, or a reservoir leaked itself empty, as empty as it wants to, any

further energy it may have is useless ; and any portion which flowed out in an uncontrolled or irreversible manner will have been wasted.

There is something specific to be said about each form of energy in order to apply the above statement definitely to that form ; and in the case of heat the supplementary statement needed is that heat will not automatically leave a body for others at higher temperature : if it goes to a hotter body it must be carried by matter, or electricity, or something else, so that it is not a pure and simple flow of heat. In other words, heat will not flow " uphill " by pure conduction ; and conduction is the only mode of automatic conveyance of heat as heat*. Water, air, or electricity can flow " uphill " for a time and can do work at the same time, by reason of their property of inertia. Heat cannot : it has no inertia. None of the uphill processes can go on continually or cyclically †.

The law of dissipation of energy states itself thus :—

If a body has any portion of energy in such condition that it is able irreversibly to leave the body, that portion usually does leave, sooner or later. This is only a rewording of the customary statement that the potential energy of a system tends towards a minimum ; or, really, except that circumstances often delay the consummation unpractically long, towards zero. The universe will be stagnant, though by no means stationary, when its potential energy is nothing. There will then be abundance of motion but no force, and therefore no " activity."

To make use of the readily detachable portion of energy may not be a very simple thing, and commonly requires a machine, sometimes an ingenious machine. Give a savage a charged Leyden jar, and he will probably detach from it its available energy pretty soon. But give him a charged storage-battery and he will not know what to do. A bit of thin

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* Radiation is not heat, but another quite distinct form of energy. The phrase " radiant heat " is responsible for immense confusion.

† Work and heat may be coaxed out of a body below the temperature of surrounding objects, as, for instance, by letting air escape from a high-pressure reservoir; but such a process is not cyclical until the air is put back again, and by that operation the heat has to be put back too. Heat by itself cannot flow uphill at all.

wire, however, is all the mechanism absolutely needed in order to afford him some light and heat ; if mechanical motion is required then some form of dynamo or electromagnetic motor must be supplied. Often and often we do not know how fairly to utilise even the portion of energy automatically streaming off from a body—from a gas-flame for instance when we need light. To utilise more of a body's energy than it will part with is impossible ; but the progress of science may conceivably teach us, not only how to utilise the whole of what bodies already freely give off, but also perhaps even how to make bodies (say molecules for instance) part with much energy which at present, if left to themselves, they permanently retain.

The first and most general portion of the second law of Thermodynamics, stated above in italics, will always remain true, even when the second part, about the non-uphill flow of heat, has by future discovery been upset ; because the application of a machine for the purpose of extracting other-wise retained energy, not by a process analogous to pumping but by enabling it automatically to flow whereas without the machine it could not, can hardly be regarded as other than automatic ; else would the present machines for directing the flow of already available heat be liable to a similar objection. Even if the contrivance necessary for extracting molecular energy turn out to be a live thing,—and this Dr. Johnstone Stoney* most suggestively conceives it possible may be the function of some bacteria [a remarkably appropriate de-moniacal function for the producers of disease], yet life, too, so far as it falls into the scheme of physics, must be con-sidered as an automatic process, and only the energy which by any device a body can be made automatically to yield without pumping can ever be utilised. But statements about heat not flowing up hill, or about not cooling bodies below surrounding objects, or about $\int \frac{dQ}{T} = 0$, or $dQ = Td\phi$, &c. ; these are liable to ultimate modification with the progress of science, since the very terms Heat and Temperature are undynamical blinkers appropriate to the consideration of particles in the lump.

* Phil. Mag. April 1893.

APPENDIX 1.—*The Objectivity of Energy and the Question of Gravitation.*

Mr. Heaviside* freely contemplates the flux of energy, but declines to admit its identity, or as he calls it objectivity. And he further doubts my proposition about transformation accompanying transference, because, he says, " convection of energy," *i. e.* simple locomotion of stored energy, "is a true flux." So it is, but it is not what I meant by *transfer.* Locomotion is so absolutely essential to translational kinetic energy that I hardly think it can be desirable ever to speak of mere locomotion as transfer, even although the moving thing be a bent bow or stretched spring. It is, however, a question of convenience, and undeniably convection must enter into a flux equation, for a bullet entering a partitioned-off region of space brings into it energy which was not there before, and, when it leaves, conveys it out again.

My proposition amounts to just this, that whatever energy appears in a bounded region must necessarily have passed through the boundary. This, if true, seems to me to confer upon energy the same kind of identity or continuous existence (or if you please objectivity) as matter possesses.

The ordinary law of conservation does not assert or contemplate continuous existence : it has no objection to seeing energy disappear from existence in one place provided an equal quantity reappears somewhere else—say inside a bounded region. Either it does not attend to or believe in the fact of transfer, or else it is satisfied with a kind of fourth-dimensional out-of-space path.

As to "objectivity" or "reality," there are always metaphysical difficulties about predicating that ; and Mr. Heaviside's objection that since motion is relative, energy can hardly be absolute, must be allowed due weight. This is a point on which Professor Newcomb has written in Phil. Mag. February 1889 ; arguing that it actually limits the generality of the law of conservation. I hope some day to discuss this at more length ; meanwhile my belief is that it will be

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* Phil. Trans. 1892, p. 427, "On the Forces, Stresses, and Fluxes of Energy in the Electromagnetic Field," by Oliver Heaviside, F.R.S.

ascertained that motion with respect to the ether is the energetic thing and that other absolute motion is meaningless.

The fact (if fact it be) that energy has a continuous existence, or that if it appears in a closed region it must have penetrated the walls, is expressed quite clearly by Mr. Heaviside's equation

$$\text{conv } eu = \dot{e},$$

where e is the energy per unit volume, u is its velocity of loco-motion, and where *conv.* stands for $S\nabla$, or $d/dx + d/dy + d/dz$.

But Mr. Heaviside is not satisfied with this simple equation of continuity, and proceeds to complicate it by introducing:—

(1) Intrinsic sources; *i.e.* creation or fourth-dimensional apparition of energy; of which the chief example is the gravitation bogey, whose path and nature no man yet knows.

(2) Flux of energy travelling not with matter at a definite speed, but in some other way so that its speed is uncertain. For example of this he instances radiation, but surely that has a definite enough velocity. He might have instanced conduction of heat; but there again, treated merely as a flux of energy, the amount crossing unit area per second is definite enough. Mr. Heaviside would probably agree, but would prefer not to analyse it into two factors e and u; and to this I cannot object.

(3) But to his third category Q, the rate of waste of energy, I am bound to object. The insertion of dissipation of energy as if it were a mysterious disappearance term, is open to the objection suggested above against (1), and also to the objection that it unduly elevates the available portion of energy into being the whole of it.

So long as these various terms are only introduced for prac-tical purposes, *i.e.* to direct attention to what might otherwise get overlooked, they are well enough; but they must not be supposed to represent the reality of things. It is true that the case of gravitation, if it be transmitted instantaneously, as seems not unlikely, is a curious simulacrum of action at a distance; whereby of course energy could be generated *de novo* inside closed boundaries readily enough; but infinite speed of transmission only requires infinite incompressibility in a medium, it does not dispense with a medium; and if a medium

of transmission exists, as all analogy and coherence urges if not insists, then gravitation is no exception, and its energy must pass through the walls in order to get inside a boundary, although it may pass through at an infinite pace. It may be better, however, not to assume the pace infinite till proved, but to have a term in the most general energy-equations expressive of the possible propagation of gravitation in time, notwithstanding that its speed is unknown and certainly excessively great.

I may refer to another reply I have made to Mr. Heaviside in ' Nature,' vol xlvii. p. 293 (January 26, 1893).

APPENDIX 2.—*More detailed Discussion of the Transmission of Energy in difficult cases.*

When I say (as I do on pp. 304, 305, and 311) that acting bodies have the same velocity, I of course mean their action to be immediate. If indirect action is contemplated, it is too obvious that a clock-weight has not the same speed as the tip of the second-hand or the hammer of its bell ; but at the contact of every cog, two wheels, driver and driven, are moving at the same pace. But now, it may be asked, if all action is contact-action, if all action is direct, how is it possible ever to get a variation in speed ? This is a question worth answering.

It is done and only done by means of rotation. The type of all such actions is a rotating wheel propelled by an uncentral force. In such a wheel, regarded as a single rigid body, we have every gradation of speed from a maximum to nothing, and we can make use of or transmit elsewhere what speed we like. This is the essence of levers, and mechanism in general : without rotation the speed of all parts is the same, and therefore the same as the point to which the driving force is applied.

But now, treating the wheel as what it is—an assemblage of particles—how comes it that they can act on each other so as to generate differences of speed ? How can a force applied tangentially to the face of a sphere cause part of the opposite hemisphere to move backwards ?

If we accept the sphere as a rigid body, nothing is easier than to equate the momentum generated to the impulse of the

applied force, and its moment of momentum to the moment
of the impulse ; but if we treat it as an assemblage of con-
nected particles it is not so easy to tackle the problem.   As
is well known it did historically give trouble, until it was
realised, on the ground of Newton's third law (or D'Alem-
bert's Principle as it was called), that all internal stresses
balanced each other, and might therefore be ignored for the
purpose of deducing the final result.

There is now no controversy as to final result ; the only
question is how universal contact-action, with equal velocity
between agent and patient, or driver and driven, can account
for the ultimate result of all grades of velocity through zero
even to minus.

There is no need to take refuge behind any such blinkers
as D'Alembert's Principle : an assemblage of connected par-
ticles can be directly contemplated.   Let one of them receive
a blow, it is passed on to the others and the momentum
spreads laterally by oblique impacts, the amount of obliquity
being limited only by the arrangement of the molecules; and
the component of the blow or thrust transmitted in any di-
rection is diminished in accordance with the suitable cosine
law.   The process is notoriously not an easy one to follow
into detail, even in a simple case, partly for lack of data ; and
there is some uncertainty as to the disposal of the energy for
the case of a blow, though even in that case there is no un-
certainty about the momentum ; while for a steady force the
body, however essentially elastic, gets rapidly into a practi-
cally rigid state, and the molecules then merely act as trans-
mitters of the energy.   They are, as it were, connected by
massless struts and ties, and along these the energy is trans-
mitted, partitioning itself off into several directions, much as
it did in the case of impact, and producing local velocities
determined by the arrangement of the particles, that is, by
the shape and other circumstances of the body*.

The conception of a rigid body, to which a couple can be
applied, and which moves as a whole without dislocation of
parts, every portion instantaneously feeling whatever force

* In Thomson and Tait, vol. i. part 1, §§ 311–318, the effect of an
impact on a number of elastically connected particles is treated.   So it is
partly in a review in 'Nature,' vol. xlvii. p. 601 (April 27, 1893).

there is to reach it, simplifies problems enormously ; and it
may be said that just as a moving body retains its kinetic
energy and carries it through space without transfer or trans-
formation, so a rigid body conveys thrust or potential energy
through space, receiving it at one point, delivering it up at
other points, and transmitting it instantaneously without
transfer or transformation. The thrust of a connecting-rod,
the torque of a shaft, the tension of a belt, and the tangential
stress of a cog-wheel, are typical instances of this practically
instantaneous communication, or locomotion of potential
energy, caused by a rigid body.

The simplest way to think of the ordinary case of gearing
and shafting is thus to ignore its molecular structure and treat
it as a linkage of entirely rigid bodies, where the potential
energy communicated to one point is conveyed elsewhere as
a simple flux without transfer or transformation, as kinetic
is conveyed when a bullet is shot across an empty space.

But that this blindfold treatment does not exhaust the
matter can be seen at once by thinking of a moving fly-wheel
suddenly geared on to a stationary cog-wheel, so as to transmit
a portion of its kinetic energy to machinery. The stress
necessary to effect the transfer is too gigantic, and results in
damage unless some elasticity is provided in shaft or spokes
wherewith to store it temporarily and subsequently give it
out again in the kinetic form, or unless by means of slip a
good deal of energy is dissipated. But some dissipation is
essential anyhow : a certain fraction, equal to the ratio of the
moving mass to the whole mass, must assume a vibrational
form of some kind.

It is instructive to recollect certain elementary facts, such
as that in the impact of two perfectly elastic bodies, one
moving, one stationary, all the energy and all the momentum
may be transmitted complete, but that in this case the
resulting motion of the two bodies cannot be the same.
Whereas, if by reason of elastic vibrations, or by reason of
heat dissipation, it is arranged that two equal bodies shall
after impact move together, then only one half the momentum
and only one quarter of the energy is transmitted, the re-
maining half of the energy having been diverted or wasted in
heat or vibration.

So it is with any system of shafting or mill-gearing to which motion has to be communicated from a revolving fly-wheel. The conservation of moment of momentum gives part of the circumstances; the refusal of ultimate mutual recoil gives the other part. And the amount of ultimate vibrational dissipation of energy is precisely the same, under these circumstances, for perfectly elastic as for perfectly inelastic bodies. The case is not unlike the opening of a stopcock between a full and empty pair of reservoirs. A certain fraction of the energy is necessarily either dissipated in heat or left as permanent vibration.
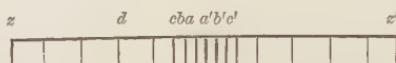
Consider the circumstance of the impact of a couple of equal elastic rods moving end-on. Conceive them in transverse strata labelled a, b, c, ... z for each rod. Let one rod be stationary, and the other strike it longitudinally with velocity v. At the beginning of the impact a strikes a', and the two move on in contact with the speed $\frac{1}{2}v$. b now strikes the mass $a\,a'$, and would accelerate it, but at the same instant a' strikes b', communicating the motion to it and neutralizing the acceleration on itself; so that now the four strata are all moving with speed $\frac{1}{2}v$, while all the rest of one rod is still stationary, and all the rest of the other rod is still moving with original velocity v. The length of the half-speed piece about the point of contact continually increases, and behaves as a body under gradually increasing compression as it receives blow after blow on either end. At length z and z' strike, and the recoil is ready to begin. This is the middle of the impact, half the momentum has been transmitted and half the energy; but of the transmitted half energy, one-half again, or a quarter of the original only is in the kinetic form, the other quarter is in the potential or elastic-stress form.

If the rods are inelastic everything so far has happened similarly, but nothing further happens; what I have called the middle of the elastic impact is the end of the inelastic one. Kinetic energy from w and y has been transmitted to w' and y' through the intervention of the quasi-rigid compressed portion of each rod, and energy from z has reached z'.

A quarter of the whole energy has been transmitted direct as kinetic, and there is no potential because there is no recoil. One-half the original energy has been lost.

If the rods are elastic the recoil brings z to rest and flings z' on with the original velocity v; then y stops and y' flies on after z', and so on, till at the end of the impact the rods part asunder, the first one completely stationary, the second one completely moving with all the energy simply kinetic.

If the rods are partially elastic some of the potential energy is dissipated and some utilised, while if they are of unequal length or material the pulses are not timed similarly in both; the shorter one (supposed the striker) is struck dead as before, the longer one is left with a pulse in it after they have separated, and its residual potential energy then assumes the sound-vibrational form : the strata progressing jerkily for some time.

If the rod to be moved is incompressible, its pulse travels instantaneously and it all gets moved at once. A blow to such a rod transmits energy instantaneously, and all in the kinetic form, but there is nothing in mere speed to affect the amount transmitted.

By the consideration of instances we have thus been led to the induction that energy can be transmitted without obvious change of form by substances with infinite properties, e. g. by an incompressible solid ; all molecular processes being either non-existent or being ignored ; but that with ordinary matter there is always some percentage of obvious transformation, though we may apparently have all grades of it from complete to very small.

Thinking of these impact cases alone, it might appear as if I had been overhasty in saying that the whole of energy must be transformed when it is transferred. Yet observe that it has to pass *through* the intermediate condition. A row of ivory balls in contact has another thrown against one end, and from the other end one leaps off. The energy has been transmitted through the row somewhat as it is transmitted through the compressed strata of two impinging rods. Yet if the elastic connexions of every stratum are attended to, and if these be regarded as massless, I think it will be found that all the transmitted kinetic has really passed through a momentary

existence as potential. The fact of necessary transformation
is not so obvious when you come to look into some of these
special cases ; but I would refer once more to the proof given
at the beginning of Part IV., which seems to me conclusive
as to essential fact.

The difficulty arises because when an elastic body is struck
(say a massive molecule with a massless spiral spring con-
nexion) it begins to move a little directly the spring is the
least compressed, and is moving half speed when the spring is
fully compressed ; but I venture to say that on any view of
the identity of energy the bit of kinetic which it first attains
is a bit of energy that has been transmitted through the
elastic stress of the spring, and that just as the second half
of the energy must admittedly exist in the spring before it
can reach the mass, so the first half has already passed through
the spring and has reached the mass only after transmutation,
although the transformation is disguised while the trans-
ference is obvious.

I have now written enough to emphasize what I want to
bring forward as the simple doctrine of energy. Some years
ago * I attempted it with brevity, but failed to make it clear
or to call proper attention to it. Now I have set it forth at
length, with illustrative cases, as matter for discussion. It
remains to try and formulate briefly and strictly the extended
laws of motion appropriate to our present knowledge.

* Especially in Phil. Mag. for October 1879, page 278 *et seq.*, for
January 1881, p. 37, and June 1881, p. 530, and for June 1885, p. 482.
Also in 'Elementary Mechanics' (Chambers), which was written in
1876 and revised about 1884, without change in the energy chapter so
far as I remember ; I am not responsible for dates on title-pages.

My attention has just been called to a " Smith's Prize " essay by Mr. R.
F. Muirhead, communicated by Professor James Thomson to the Philo-
sophical Magazine for June 1887. This essay, both in itself and in it
numerous quotations and criticisms, is an instructive and useful summary
or exposition of nearly everything that is foggy, confusing, and utterly
unsatisfactory in the fundamental treatment of Dynamics. It is hardly
too much to say broadly that the entire order of ideas in that essay is
antipodal to the conceptions I am endeavouring to urge on the acceptance
of Physicists.

Discussion.

Prof. Greenhill said the paper was full of suggestive ideas. He could not agree with all the views expressed in the paper on the subject of Newton's 3rd law. In considering action and reaction as equal and opposite, Newton ignored the inertia of the medium, and if this be included, the forces are no longer equal.

Mr. Boys asked Prof. Greenhill if the first link of a chain pulled on the second link harder than the second pulled on the first?

Prof. S. P. Thompson thanked the author for getting rid of all square laws. He himself could not conceive of any effect being more than proportional to the cause.

Mr. Burbury, referring to Dr. Lodge's deduction of conservation of energy on slip 6 of the paper, pointed out that two bodies may act on one another with equal and opposite forces and yet be moving with unequal velocities, and that the kinetic energy of such a system is not conserved. He did not see that denial of action at a distance was necessary to the proof given by the author.

Dr. Stoney said that only provisional and temporary foundations of dynamics could be deduced in the way followed by Dr. Lodge. It was necessary to first ascertain how the study of Nature is related to Ontology, but this essential the author passed over by discarding metaphysics. When pursued in relation to the scientific study of Nature, Ontology throws a flood of light on most of the points dealt with in the paper.

Prof. Herroun considered that universal contact-action involves the conception of a homogeneous ether filling all space, and yet having ordinary matter dispersed through it; this he regarded as a metaphysical impossibility. If the medium be supposed molecular, then action at a distance was only postponed one step. He thought Mr. Heaviside was right in denying the objectivity of energy, for it had no more claim to be regarded as an entity than " life " or " death," " hardness " or " colour," or any other generalized abstraction.

Prof. Minchin said the first fundamental axiom of dynamics postulates the existence of *force* as an entity distinct

from *matter*, *space*, and *time*, and this was the object of Newton's First Law. It also gave the criterion of the presence of force. To merely retain the law as defining *equal times* was to degrade it. As regards the supposed impossibility of defining uniform motion, he said similar difficulties occur in all sciences, even in geometry. Nevertheless, a rational science of geometry existed. In dynamics we had notions of a right line and of uniform motion in it, although no criterion of either may exist. The fact that the science harmonises with ordinary experience constitutes its validity. In his opinion the extraordinary devices which had been suggested for defining directions fixed in space were unnecessary, and merely served to cover the subject with ridicule.

He disagreed with Prof. Lodge in admitting the first law as a particular case of the second, for unless force was postulated (the function of the first law) the second became a mere definition, and not a law. Speaking of the third law, he said the author had made a serious error in stating that it could be deduced from the first, for the centre of mass of a system might be at rest without action and reaction necessarily being equal and opposite. The third law was not superfluous ; neglecting it had led to great misconception and mystery about the principle of virtual work, and D'Alembert's principle, both of which are simple deductions from it.

In opposition to Dr. Lodge, he defended the ordinary definition of energy, and asserted that without the notion of *force* and *work*, the term *energy* loses all meaning.

Speaking of transference and transformation of energy, he inquired if the proof given could be applied to the case of a body sliding down a rough, rigid, inclined plane, for here the stress (friction) does work on the body but not on the plane, and there was no transference. He regretted that the expression " potential energy " was used in different senses in the paper, sometimes meaning " static energy," and at others "the available portion of the kinetic energy of a body."

Referring to the idea of all energy being ultimately kinetic, he asked if, by accepting this, the author meant to surrender the independent existence of force. If so, difficulties would arise ; for example, in the kinetic theory of gases the ex-

pression for the pressure, $p = \frac{1}{3}\rho v^2$, was only arrived at by assuming the existence of force. The statement on the top of slip 9 about making a "moving body do work" was not necessarily true, as might be seen by considering the case of a sphere rolling down a rough inclined plane.

Prof. O. Henrici thought axioms should be treated as true logical definitions, as for example in geometry. "Two straight lines cannot enclose a space." Every new notion required its axiom. In passing from geometry to kinematics, the idea of time presented itself, and the appropriate axiom was contained in Newton's first law. On approaching dynamics, force and mass were met with. He disagreed with Prof. Minchin in regarding force as most fundamental. Mass was more essential, for force might be abolished. On the other hand, he concurred with Prof. Minchin in thinking that the idea of a centre of mass was not axiomatic. Referring to Dr. Lodge's summary ('Nature,' May 18, 1893), he agreed with axiom (a) fully, and with (b) partially. Axiom 3 required further development. The critical point, however, was axiom 4 : "Stress cannot exist in or across empty space." This he regarded as very incomplete, and maintained that axioms defining the properties of the ether were necessary to further progress. If varieties of space be contemplated, each advance required fresh axioms.

Dr. C. V. Burton remarked that contact movement did not necessitate equal velocities ; sliding motion was a case in point. Again, in deforming an incompressible fluid, although force and motion might exist, no work was done. Conservation could not be proved from denial of action at a distance. Speaking of the doctrine of transference and transformation of energy, he said it was a convenient working rule, but not true universally. Newton's laws were simple and consistent, but some doubt existed as to how much was definition and how much *law*, or fact.

Mr. Founier d'Albe disagreed with Prof. Lodge in regarding the conception of uniform motion as a primary muscular sensation. It would be more correct to say that the conception was based on optical sensation, whilst the idea of force was derived from muscular sense. Newton's first law was not an axiom, for it could be proved experimentally.

He thought Prof. Lodge's criterion of the identity of energy quite sufficient, if taken in conjunction with its conservation. The difference between the identity of energy and of matter lay in the number of attributes by which they could be identified, energy having only one, viz., quantity.

Prof. Ayrton said the best foundations of dynamics depended on what was most easy to grasp. On this point great difference of opinion existed; some persons thought the idea of *force* more simple than those of *mass* or *time*, whilst others had contrary impressions. He could not admit that the conservation of energy could be deduced from denial of action at a distance. Experiments were necessary. In addition to the case previously mentioned of a body sliding on a plane, he thought a hard magnet acted on by a coil approaching it and conveying a current was one in which Prof. Lodge's law of transference and transformation did not hold.

Mr. Swinburne protested against difference between theory and a working hypothesis being overlooked. All conceptions were based on experience, and ideas of ether and atoms derived from "jelly" and "cricket balls." We ought also to remember what "explanation" means, viz., describing the unfamiliar in terms of the more familiar. It was customary to describe the phenomena of fluids by reference to solids, because we were more familiar with solids; an intellectual fish would probably do the reverse. The so-called "theory of magnetism" which breaks up a bar of iron into a number of small pieces, each possessing the properties of the original bar, he regarded as absurd. It was no "explanation" and not a "theory." Ether might be used as a working hypothesis, but must not be treated as an entity.

Mr. Blakesley questioned whether transference of energy was always accompanied by transformation, and he did not see why energy should be looked upon as

$$(mv) \cdot \frac{v}{2},$$

in preference to any other subdivision of the factors. As regards effects being proportional to their causes, he pointed out that the variable part of the E.M.F. at the junction of two metals varies as the square of temperature measured

from a fixed point, and therefore this E.M.F. was always in the same direction.

Prof. S. P. Thompson, referring to the demonstration of the law of transference, &c., given on slip 8, said that attempts to translate it into Latin or Greek at once revealed the ambiguous character of the proof. Speaking of Ohm's law, he pointed out that R, a constant, was not an essential feature as Dr. Lodge supposed. Ohm never said R was constant.

In identifying energy, a difficulty presented itself, for one never came across it as a single thing, but as a product, and in being transformed the paths of the two factors might possibly be different.

Mr. Dickson said the whole of geometry and dynamics could be based on verbal definitions. The conservation of energy could be written as

Kinetic energy + potential energy = a constant,

but on substituting the expressions for kinetic and potential energies, an identity resulted ; therefore the original statement was not a law. Both the kinetic and potential energies of a system were functions of its configuration. Potential energy could not belong to a particle but to a system.

The President doubted whether Dr. Lodge's scheme was more simple, natural, and logical than the ordinary one. The statement in 'Nature' (May 18th) that "strains were proportional to stresses" was simple enough, but it was questionable if "frequency of vibration is independent of amplitude" could be considered so. The author appeared to ignore *mass* in comparison with *force*, whereas the idea of *mass* seemed the more simple one. Referring to the identity of energy, he said that however far we trace it, we cannot identify its parts in molecular structure. He objected to carrying too far the ideas derived from the matter in mass to particles, and pointed out that by so doing the difficulty was only pushed one step further and not cleared up.

Dr. Lodge, in reply to Mr. Burbury, said two bodies never do attract one another ; the thing which acted on either was the medium immediately in contact with it. Mr. Herroun had used metaphysical arguments against ether, but he (Prof.

Lodge) thought it was a good thing to investigate ether.
He agreed with what Prof. Minchin said about force and
the first law of motion. *Force* was the more fundamental,
but *mass* was best as a standard unit. As regards ether, he
was prepared to say that it has no motion. It possessed
electromagnetic kinetic energy, and probably all the stress
energy that exists. Referring to the slipping body mentioned
by Prof. Minchin and Dr. Burton, he said that in speaking
of the velocities of acting and reacting bodies being equal,
he always meant that their velocities along the line of action
were equal. The action between the sliding body and plane
was a " catch and let go " one, like a fiddle-bow and string.
On the second law of thermodynamics he hoped to say some-
thing in a subsequent paper. When he spoke of R being
constant as the essence of Ohm's law, he meant constancy as
regards terms which appear in the equation

$$\frac{E}{C} = R.$$

XIX. *On the Methods of Theoretical Physics.*
By Ludwig Boltzmann.

CALLED upon by the Editors of the Catalogue to deal with
this subject, I soon became aware that little which is new
could be said, so much and such sterling matter having in
recent times been written about it. An almost exaggerated
criticism of the methods of scientific investigation is indeed
a characteristic of the present day ; an intensified Critique of
.Pure Reason we might say, if this expression were not perhaps

somewhat too presumptuous. It cannot be my object again to criticise this criticism. I will only offer a few guiding remarks for those who, without being specially occupied with these questions, nevertheless take an interest in them.

In mathematics and in geometry it was at first undoubtedly the necessity for economizing labour which led from purely analytical to synthetical methods, as well as to their illustration by models. Even if this necessity appears to be a purely practical and obvious one, we here find ourselves on ground on which a whole species of modern methodological speculations have grown up, which have been expressed by Mach in the most definite and ingenious manner. He, indeed, directly maintains that the sole object of Science is economy of labour.

Seeing that in business affairs the greatest economy is desirable, it might, with equal justice, be maintained that this is simply the object of the sale-room, and of money in general, which in a certain sense would be true. Yet when we search into the distances, the motions, the magnitudes, the physical and chemical structure of the fixed stars, when microscopes are invented and we thereby discover the origins of disease, we shall not be very apt to describe this as mere economy.

But it is after all a matter of definition what we denote as an object, and what are the means for obtaining that object. It in fact depends on our own definition of existence what we recognize as existing ; whether substances, or their energy, or, in general, their properties, so that we may perhaps at last define away even our own existence.

But let this pass ; the necessity exists for the most complete utilization of our different powers of conception ; and since it is by aid of the eye that the greatest mass of facts can be grasped simultaneously, it becomes desirable to make the results of our calculation perceptible, and that not merely by the imagination, but visible to the eye and at the same time palpable to the touch by means of gypsum and of cardboard.

How little was done in this direction in my student days! Mathematical instruments were almost wholly unknown, and physical experiments were often made in such a manner that they could only be seen by the Lecturer himself. And as, further, owing to shortness of sight I was unable to see

writing on the blackboard, my imagination was constantly
kept on the stretch. I had almost said to my good fortune.
Yet this latter statement would be in opposition to the object of
the present Catalogue, which can only be to praise the infinite
equipment of models in the mathematics of the present day ;
and it would, moreover, be quite incorrect. For even if my
powers of conception had gained, it could only have been at
the expense of the range of my acquired knowledge. At
that time the theory of surfaces of the second order was still
the summit of geometrical knowledge, and an egg, a napkin
ring, or a saddle was sufficient. What a host of shapes,
singularities, and of forms growing organically out of each
other, must not the geometrician of the present day impress
on his memory ! and how greatly is he not helped by plaster
casts, models with fixed and movable strings, links, and all
kinds of joints !

Not only so, but those machines make more and more way
which serve not for mere illustration, but save the trouble of
making actual calculations, from the four ordinary rules of
arithmetic to the most complicated integrations.

As a matter of course both kinds of apparatus are most ex-
tensively used by physicists, who, in any case, are continually
accustomed to the manipulation of all kinds of apparatus.
Optical wave-surfaces, thermodynamical surfaces in gypsum,
wave-machines of all kinds, apparatus for illustrating the laws
of the refraction of light and other laws of nature, are
examples of models of the first kind.

In the construction of instruments of the second kind some
have even gone so far as to attempt the evaluation of the
integrals of differential equations which hold equally for a
phenomenon difficult to observe (like the friction of gases)
and for another which allows of easy measurements (like the
distribution of an electric current in a conductor of suitable
shape) by observations of the latter, and then to utilize these
values for the determination of the constant of friction.
We may also remember the graphical utilization of the series
and integrals occurring in the theory of tides, in electro-
dynamics by Lord Kelvin, who, in his 'Lectures on Molecular
Dynamics,' suggests even the establishment of a mathematical
institution for such calculations.

In theoretical physics other models are more and more coming into use which I am inclined to class as a third species, for they owe their origin to a peculiar method which is being applied more and more in this science. I believe this is due rather to practical physical needs than to speculations as to the theory of cognition. The method ha nevertheless, an eminently philosophical stamp, and we must accordingly enter afresh the field of the theory of cognition.

At the time of the French Revolution and afterwards the great mathematicians of Paris had built up a sharply defined method of theoretical physics on the basis laid by Galilei and Newton. Mechanical assumptions were made, from which a group of natural phenomena could be explained by means of mechanical principles which had attained a kind of geometrical evidence. Men were conscious that the assumptions could not with complete certainty be described as correct, yet up to a certain point it was held to be probable that they were in exact conformity with fact, and accordingly they were called hypotheses. Matter, the luminiferous æther for explaining the phenomena of light, and the two electrical fluids as sums of mathematical points were thus conceived. Between each pair of such points a force was supposed to act having its direction in the line joining the two points, and whose strength was a function of their distance, still to be determined. (*Boscovich.*)

An intellect knowing all the initial positions, and initial velocities of all these material particles, as well as all the forces, and which could integrate all the differential equations arising out of them, would be able to calculate beforehand the whole course of the universe just as the astronomer can predict a solar eclipse. (*Laplace.*)

There was no hesitation in declaring those forces, which were accepted as axiomatically given and not further explainable, to be the causes of the phenomena, and the determination of their values by aid of their differential equations to be their explanation.

To this was afterwards added the hypothesis that even in bodies at rest their particles are themselves in a state of motion, which give rise to thermal phenomena, and whose

nature is especially sharply defined in the case of gases.
(*Clausius.*)

The theory of gases led to surprising prognostications ;
thus, for instance, that the coefficient of friction was independ-
ent of the pressure, and certain relations between friction,
diffusion, and conductivity for heat, &c., &c.   (*Maxwell.*)

The aggregate of these methods was so successful that to
explain natural phenomena was defined as the aim of natural
science ; and what were formerly called the descriptive natural
sciences triumphed, when Darwin's hypothesis made it possible
not only to describe the various living forms and phenomena,
but also to explain them.   Strangely enough Physics made
almost exactly at the same time a turn in the opposite
direction.

To Kirchhoff, more especially, it seemed doubtful whether
it was justifiable to assign to Forces that prominent position to
which they were raised by characterizing them as the causes
of the phenomena.

Whether, with Kepler, the form of the orbit of a planet and
the velocity at each point is defined, or, with Newton, the force
at each point, both are really only different methods of de-
scribing the facts ; and Newton's merit is only the discovery
that the description of the motion of the celestial bodies is
especially simple if the second differential of their coordinates
in respect of time is given (Acceleration, Force).   In half a
page forces were defined away, and physics made a really
descriptive natural science.   The framework of mechanics
was too firmly fixed for this change in the external aspect to
have any effect on the interior.   The theories of elasticity
which did not involve the conception of molecules were of
older date (*Stokes, Lamé, Clebsch*).   Yet in the development
of other branches of physics, Electrodynamics, theories of
pyro- and of piezoelectricity, the view gained ground that it
could not be the object of theory to penetrate the mechanism of
Nature, but that, merely starting from the simplest assump-
tions (that certain magnitudes are linear or other elementary
functions), to establish equations as elementary as possible
which enable the natural phenomena to be calculated with
the closest approximation ; as Hertz characteristically says,
only to express by bare equations the phenomena directly

observed without the variegated garments with which our imagination clothes them.

Meanwhile several investigators had, from another side, assailed the old system of centres of force and forces at a distance; it might be said to have been from the exactly opposite side, because they were particularly fond of the variegated garment of mechanical representation; it might also be said to be from an adjacent side, as they also dispensed with the recognition of a mechanism lying at the basis of the phenomena, and in the mechanism which they themselves invented they did not see those of Nature, but mere images and analogies*. Several men of science, following the lead of Faraday, had established a totally different conception of Nature. While the older system had held the centres of force to be the real, and the forces themselves to be mathematical conceptions, Faraday saw distinctly the continuous working of the forces from point to point in the intermediate space. The Potential, which had hitherto been only a formula for lightening the work of calculation, was for him the

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* The relation of the old system of centres of force, and of forces at a distance to the purely mathematical one represented by Kirchhoff, and to Maxwell's own point of view is expressed by him in the following words:—"The results of this simplification may take the form of a purely mathematical formula (*Kirchhoff*), or of a physical hypothesis (*Poisson*). In the first case we entirely lose sight of the phenomena to be explained, and though we may trace out the consequences of given laws, we can never obtain more extended views of the connexions of the subject. If, on the other hand, we adopt a physical hypothesis, we see the phenomena only through a medium, and are liable to that blindness to facts and rashness in assumption which a partial explanation encourages. We must therefore discover some method of investigation which allows the mind at every step to lay hold of a clear physical conception without being committed to any theory in physical science from which that conception is borrowed, so that it is neither drawn aside by analytical subtleties, nor carried beyond the truth by a favourite hypothesis."

Compare the theory of elasticity worked out by Kirchhoff in his Lectures, of almost etherial delicacy, clear as crystal but colourless, with that given by Thomson in the third volume of his Mathematical and Physical Papers, a sturdy realistic one, not of an ideal elastic body but of steel, india-rubber or glue; or with Maxwell's language, often almost childlike in its naïveté, who, right in the middle of his formulæ, casually gives a really good method of removing grease-spots.

really existing bond in space, the cause of the action of force itself. Faraday's ideas were far less lucid than the earlier hypotheses, defined as they were with mathematical precision, and many a mathematician of the old school had but a low opinion of Faraday's theories, without, however, by the clearness of his own conceptions making such great discoveries.

But soon, and especially in England, it was attempted to get as plain and tangible a representation of the ideas and conceptions which before had played a part in Analysis only. From this endeavour towards clearness arose the graphical representations of the fundamental conceptions of mechanics in Maxwell's 'Matter and Motion,' the geometrical representation of the superposition of two sine motions, and all illustrations due to the theory of quaternions, for instance the geometrical interpretation of the symbol

$$\Delta = \frac{d^2}{dx^2} + \frac{d^2}{dy^2} + \frac{d^2}{dz^2}. ^*$$

There was another matter. The most surprising and far-reaching analogies were seen to exist between natural phenomena which were apparently quite unlike. Nature seemed in a certain sense to have built up the most diversified things after exactly the same pattern; as the analyst dryly observes, the same differential equations hold for the most diversified phenomena.

Thus the conductivity of heat, diffusion, and the propagation of electricity in conductors takes place according to the same laws. The same equation may be considered as the solution of a problem in hydrodynamics or in the theory of potential. The theory of vortices in fluids as well as those of the friction of gases exhibits the most surprising analogy with that of electromagnetism, &c., &c. Compare on this point Maxwell, 'Scientific Papers,' vol. i. p. 156.

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* Maxwell, 'Treatise on Electricity and Magnetism,' 1873, vol. i. art. 29 : on the nature of the operator ∇ and ∇^2; this was also afterwards observed by others. Mach on M. Guebhard's representation of Equipotential Curves, *Wien. Sitzungsbericht*, vol. lxxxvi. p. 8, 1882. Compare also Riemann, 'Electricität und Magnetism," *Wied. Beiblätter*, vol. vii. p. 10 ; *Comptes Rer dus*, vol. xcv. p 479.

Maxwell also, when he undertook the mathematical treatment of Faraday's conceptions, was from the very outset impelled by their influences into a new path.   Thomson [*] had already pointed out a series of analogies between problems in the theory of elasticity and those of electromagnetism.   In his first paper on Electricity Maxwell [†] explained that it was not his intention to propound a theory of electricity : that is, that he himself did not believe in the reality of the incompressible fluids and of the resistances which he there assumes, but that he simply intends to give a mechanical example which shows great analogy with electrical phenomena, and he wishes to bring the latter into a form in which the understanding can readily grasp them [‡].

In his second paper he goes still further, and from liquid vortices, and friction wheels working within cells with elastic sides, he constructs a wonderful mechanism which serves as a mechanical model for electromagnetism.   This mechanism was of course mocked at by those who, like Zöllner, regarded it as an hypothesis in the older sense of the word, and who thought that Maxwell ascribed to it a real existence : this he decidedly repudiates, and only modestly hopes " that by such mechanical fictions any one who understands the provisional and temporary character of this hypothesis will find himself rather helped than hindered by it in his search after the true interpretation of the phenomena."   And they were so helped ; for by his model Maxwell arrived at those equations whose peculiar, almost magical power Hertz, the person most of all competent to judge, thus vigorously depicts in his lecture on the relations between Light and Electricity :—" We cannot study this wonderful theory without at times feeling as if an independent life and a reason of its own dwelt in these mathematical formulæ ; as if they were wiser than we were, wiser even than their discoverer ; as if they gave out more than had been put into them."   I should like to add to these words of Hertz only

[*] Cambridge and Dublin Math. Journal, 1847 ; Math. and Phys. Papers, vol. i.

[†] Maxwell, on Faraday's Lines of Force. Cambridge Phil. Trans. vol. x ; Scient. Papers, vol. i. p. 157.

[‡] Maxwell, Scientific Papers, vol. i. p. 157.

this, that Maxwell's formulæ are simple consequences from his mechanical models ; and Hertz's enthusiastic praise is due in the first place, not to Maxwell's analysis, but to his acute penetration in the discovery of mechanical analogies.

It is in Maxwell's third important paper * and in his Text-book that the formulæ more and more detach themselves from the model, which process was completed by Heaviside, Poynting, Rowland, Hertz, and Cohn.   Maxwell still uses the mechanical analogy, or, as he says, the dynamical illustration.   But he no longer pursues it into detail, but rather searches for the most general mechanical assumptions calculated to lead to phenomena which are analogous to those of Electromagnetism.   Thomson was led, by an extension of the ideas which have already been cited, to the quasi elastic and the quasi yielding æther, as well as to its representation by the gyrostatic-adynamic model.

Maxwell of course applied the same treatment to other branches of theoretical physics.   As mechanical analogies may be cited Maxwell's gas molecules which repel each other with a force inversely proportional to the fifth power of their distance, and at first investigators were not wanting who, not understanding Maxwell's tendency, considered his hypothesis to be improbable and absurd.

The new ideas, however, gradually found entrance into all regions.   In the theory of heat I need only mention Helmholtz's celebrated memoirs on the mechanical analogies of the second law of thermodynamics.   It was seen indeed that they corresponded better to the spirit of science than the old hypotheses, and were also more convenient for the investigator himself.   For the old hypotheses could only be kept up as long as everything just fitted ; but now a few failures of agreement did no harm, for it can be no reproach against a mere analogy if it fits rather loosely in some places.   Hence the old theories, such as the elastic theory of light, the theory of gases, the schemes of chemists for the benzole rings, were now only regarded as mechanical analogies, and philosophy at last generalized Maxwell's ideas in the doctrine that cognition is on the whole nothing else than the discovery of analogies.

* Maxwell. " A Dynamical Theory of the Electro-magnetic Field." Scientific Papers, i. pp. 526 ; Roy. Soc. Trans. vol. 155. p. 459, 1865.

With this the older scientific method was defined out of the way, and Science now only spoke in parables.

All these mechanical models at first existed indeed only in idea ; they were dynamical illustrations in the fancy, and they could not be carried out in practice, even in this general form, yet their great importance was an incitement to realize at any rate their fundamental types.

In the second part of this Catalogue is a description of such an attempt made by Maxwell himself, and of one by the author of these lines. Fitzgerald's model is also at present in the Exhibition, as well as Bjerknes' model, which owe their origin to similar tendencies. Other models which have to be classed with these have been constructed by Oliver Lodge, Lord Rayleigh, and others.

They all show how the new tendency to relinquish perfect congruence with Nature is compensated by the more striking prominence of points of similarity. To this belongs the immediate future ; yet, mistaken as it was to consider the old method as the only correct one, it would be just as one-sided to consider it, after all it has accomplished, as quite played out, and not to cultivate it along with the new one.

XX. *On the Drawing of Curves by their Curvature.*
By C. V. Boys, *A.R.S.M., F.R.S.*[*]

[Plate III.]

WHILE giving a course of lectures to working men on Capillarity at the Royal School of Mines in 1891, I wished to explain the principles upon which the form of a water-drop depends, and to show that an accurate scale-drawing could actually be produced by following Lord Kelvin's rule (Proc. R. Inst. Jan. 29, 1886) ; but found that the procedure was so cumbersome as to be in no way adapted to popular exposition. In my attempt to simplify the operation I devised a method of carrying it out which has the double advantage that any of these capillary surfaces of

* Read May 12, 1893.

revolution can be drawn with a facility which cannot be approached by following Lord Kelvin's instructions explicitly, while the accuracy of any curve thus determined is so great that it may be used for the purpose of approximate numerical calculation when, as is often the case, the equation cannot be solved. The accuracy is certainly considerably in excess of that which can be attained by the same degree of care, using rule and compasses according to his instructions. I hope therefore that though there is no principle involved which is not perfectly well understood, the practical character of the modification and its utility more especially to teachers are such as to render it worthy of the attention of the Physical Society.

Lord Kelvin's rule for drawing the generating curve of any capillary surface of revolution is as follows :—" Through any point, N, fig. 1 (Pl. III.), of the axis draw a line, NP, cutting it at any angle. With any point, O, as centre on the line NP, describe a very small circular arc through PP', and let N' be the point in which the line of OP' cuts the axis. Measure NP, N'P', and the difference of levels between P and P'. Denoting this last by δ, and taking α as a linear parameter, calculate the value of

$$\left(\frac{\delta}{\alpha^2} + \frac{1}{\mathrm{OP}} + \frac{1}{\mathrm{NP}} - \frac{1}{\mathrm{N'P'}} \right)^{-1}.$$

Take this length on the compasses, and putting the pencil-point at P', place the other point at O' on the line P'N', and with O' as centre describe a small arc, P'P''. Continue the process according to the same rule, and the successive very small arcs so drawn will constitute a curved line, which is the generating line of the surface of revolution inclosing the liquid, according to the conditions of the special case treated."

This can be explained to those not already familiar with the principle in a few words. At any depth below the plane surface level of a liquid there is a hydrostatic pressure which is proportional to the depth. At every point of the surface of a drop this is balanced by the surface-tension which is constant over the surface multiplied by the total curvature. The total curvature at any point P in a surface of revolution is defined

as being equal to $\dfrac{1}{\text{OP}} + \dfrac{1}{\text{NP}}$, where OP is the radius of curvature of the generating curve at the point P, and NP is the distance normal to the curve from P to the axis of revolution.

Since at any depth the curvature of the tense surface withstands the hydrostatic pressure and since the hydrostatic pressure is proportional to the depth, it is clear that the total curvature measured as defined above must be proportional to the depth, being concave to the liquid below and convex above the plane surface level. At any depth d the curvature c must equal $\dfrac{\Delta d}{t}$, where Δ is the density of the liquid (or difference of density of the two liquids if one forms a drop in the other), and t the surface-tension measured in gravitation units. Thus if Δ means grains per cubic inch, t is grains per linear inch. If a bubble is formed on a ring or between two rings, the effect of gravity is so minute that the curvature is practically the same at all levels, and the curves that are formed are the well-known roulettes of the foci of the conic sections, whose equations are known.

In order to avoid the loss of time which results from the perpetual finding of reciprocals in order to determine the new radius of curvature of each small step in the curve, I have divided a rule so that the distances of the divisions from the beginning of the scale are the reciprocals of the numbers attached to them. Such a rule is adapted to measure the smallness of a thing just as an ordinary rule measures its bigness or size. Thus a large thing since it has very little smallness is found by the rule to be measured by a very small number, while a very little thing having a considerable smallness is measured in the same way by a very large number. The curvature of a line is measured by the reciprocal or the smallness of its radius, and so the curvature of a line is read off immediately by the rule. The curvature of a surface is measured by the smallness of two lines which start at the same point and in the same or in opposite directions, and so the curvature of a surface of revolution is found by inspection of the two curvatures upon the rule. In order to be equally ready to deal with the case when the lines are measured in

opposite directions, and with a further object not yet apparent, I place the zero of the scale, marked ∞, somewhere about the middle and divide it each way. Then, if the two radii are found on the same side of the ∞ or centre, their values are to be added, and if on the opposite side one is to be subtracted from the other in order immediately to find the total curvature. The only gain apparent at present is the great saving of time resulting from an obvious construction. The increase in accuracy depends upon the abolition of all accumulated errors of compass setting, which are of three kinds :—(1) The compass may not be set to the exact radius required ; (2) the pencil- or pen-point may not be placed exactly upon the end of the line just drawn, producing a step ; and (3) the needle-point may not be set exactly upon the former radius-line, producing an angle invisible no doubt, but still existent in the curve. The first of these applies to the method of this paper, but in less degree; the second and third are as nearly perfectly eliminated as is possible. These, though they may be individually too minute to be observed, yet tend to be cumulative, so that as the number of steps employed is increased with the view to obtain accuracy, so do these accumulated errors tend to increase when the ordinary method is followed. It is on the elimination of these that the accuracy of the method of curve-drawing, which forms the subject of this paper, mainly depends. The increase in facility is also of importance, as the operator is not tempted to make his steps unduly long ; it is brought about as follows. I make the rule of a thin strip of transparent celluloid with a sma l hole at the centre of the scale marked ∞. A small brass tripod with three needle-feet (fig. 2) is placed so that two feet just penetrate into the paper and the third rests on the longitudinal straight line of the strip (which of course passes through the small hole), and just pricking into it forms a temporary but rigid and stable centre of rotation for the strip. A pencil-point or a capillary glass pen made short and fastened with wax or a spring clip into its place at ∞ will then draw an arc of a circle of which the curvature is equal to the reading on the strip at the place where the needle-point presses. Now it will be clear that at every stage the two numbers which represent the two parts of the

curvature are always visible to the eye, there is no necessity
to rule any of the normals to the curve, and no necessity to
set compasses. Supposing for simplicity that a curve of
constant total curvature (unduloid, nodoid, or catenoid), that
is where hydrostatic pressure need not be considered, is to be
drawn through any point and normal to any definite direction.
Then all that has to be done is to set the strip with its longi-
tudinal line in this direction and with its central hole at the
point. $E.\,g.$, let the point be 6 inches from the axis, let the
line through the point to which the curve is to be normal be
perpendicular to the axis, and let the total curvature be
arbitrarily chosen as ·5. Then when the strip is placed in
position the reading on the scale where it crosses the axis
will be ·1$\dot{6}$. The difference between ·5 and ·1$\dot{6}$ is ·$\dot{3}$. The
needle-point must therefore be set on the scale at ·$\dot{3}$, and on
the side of ∞ towards the axis. A very short arc must then
be drawn with the pencil or pen piercing the hole at ∞. The
reading on the axis will now be found very slightly less (the
distance being greater), say ·16, the needle-point must now
be transferred to the ·34, care being taken to hold the strip
firmly upon the paper to prevent its moving. A new short
arc may now be drawn and the process continued. As soon
as the curve becomes notably inclined to the axis the change of
reading on the axis between the two ends of a short arc even
may be more than appreciable, so that if the process were
carried out exactly as described the needle-point would be on
the whole too late in making its move ; $i.\,e.$ where the radius
is increasing it would be too small, and $vice\ vers\hat{a}$. To avoid
this it is convenient to choose such a small arc as shall change
the axial reading by any small number of divisions at a time
and to move the needle-point through a corresponding number
at a time also, but so that the needle-point is placed always
at that division which corresponds with the intermediate one of
the axial divisions. The curve can in this way be carried on very
rapidly by very small steps without ever even looking to see
where the pencil or pen is being taken, and it in no way suffers
from the small errors of each resetting of an ordinary compass.
It is not even necessary to draw the curve at all. The process
can be carried out as described, and the curve will be traced
as in imagination by the hole at ∞. The perfection of the

result which is obtainable, whether the compass or the reci-
procal rule method is employed, depends of course upon the
fact that whereas a very fair representation of a curve may be
made by means of a polygon with a very great number of very
short sides, each making very nearly an angle of two right
with its neighbour, a vastly more perfect result is produced by
a corresponding series of small arcs of circles, each having the
necessary radius of curvature and making no angular break
at each stage.   The small discontinuity is one of curvature
only, not of direction.   The gain in smoothness and accuracy
is still very great when the steps are made larger, so that a
polygon of the same number of sides would cease to fairly
represent the curve.   Where the curvature is a maximum
or a minimum the contact of the circle of curvatures is of the
third instead of the second order, and at these parts a much
longer step may be taken without impairing the accuracy of
the curve.   Similarly, if instead of the arc of curvature being
used for each small step some curve of curvature and change
of curvature could be employed, a still closer approximation
would result, or still larger steps could be safely employed.

Going back now to the curve which has been chosen (a
nodoid), it will be found that the point on the axis gets
gradually farther away until the end of the rule is reached at
·05 in the case of the rule exhibited.   The needle-point will
now be at division ·45.   For the next two or three steps the
strip will be rapidly approaching parallelism with the axis,
so that the axial reading is equally rapidly approaching 0 ;
i. e. at an infinite distance.   During these steps the axial
reading cannot be directly observed ; but since by the time
that the rule is parallel to the axis the needle-point must
have reached, in the particular case chosen, the division ·5,
the two or three steps required may be made with more
than sufficient accuracy by moving the needle-point a few
divisions at a time so as to arrive at this point when
parallelism is being reached ; in fact at this stage equal
angular changes correspond to equal changes on the scale.
As soon as this position is passed the central line of the strip
cuts the axis on the other side, and so now these readings
have to be added to the total curvature ·5 in order to find the
remaining readings upon which the needle-point should rest.

In fig. 2 one complete loop of this particular nodoid has been
drawn, and the second one has been carried to the stage at
which ·16 is the axial reading, and ·34 the reading of the
temporary centre of curvature.   Unduloids may be drawn in
a similar way, but in this case the needle-point has to move
off to an infinite distance, when the axial reading is equal to
the total curvature.   The length of line for which the centre
of curvature is beyond the end of the rule is generally very
small, and this may be drawn so that no error can be detected
by alternately moving the needle from one end to the other,
each for a very small arc.   The catenary may of course be
set out in the same way and with still greater ease and accu-
racy, for the infinite values do not come into any finite part
of the curve.   The axial reading and that on which the needle-
point rests must be kept the same, but on opposite sides of the
tracing-point.

It will be evident that if the little tripod is just pressed
sufficiently upon the paper to make small impressions of the
needle-feet, it may afterwards be taken over the same course
and the third needle-point pressed upon the paper, and thus a
series of points on the evolute determined.   In fig. 2 the evolute
is shown dotted.   The points of most importance on this are
the cusps a, b, corresponding to the places on the curve where
the curvature is a minimum or a maximum, the distance $a b$
being the half period of the curve, and the maxima or minima
c, c, which are the ordinates of the curve where the abscissa
is a minimum or a maximum.   By means of these and the
curve itself the geometrical constants of any curves drawn in
this way may be determined more accurately than from the
curve itself alone.   Prof. Greenhill has given me the formula
by which, with the use of tables of elliptic integrals, the chief
constants of the unduloid and nodoid may be calculated.
Miss Stevenson, a student of the College of Science, has drawn
a large series of these curves, and we have found that while
they can be drawn each in a few minutes only, it is possible
to set them out correctly to the thickness of a fairly fine
pencil-line; and so they do more than give a general idea of
the shape, they serve to make a true scale-drawing.

In the case of capillary surfaces of revolution, in which the
fluids on the opposite sides of the surface are of different den-

sity, *e. g.* water and air as already explained, the curvature of
the surface must be zero at the plane surface level, and must
be proportional to the distance above or below this level, being
— or convex to the denser liquid above, and + or concave
below. The readiest way to determine the curvature when
drawing one of these generating curves is to work on a sheet on
which a series of equidistant lines have been ruled parallel
to the plane surface level, and at such a distance apart that
the hydrostatic pressure due to this distance is balanced by a
curvature of ·01 or some small and simple number. These lines,
then, give by inspection, especially if each fifth and tenth line
is a little darker, a scale of curvature, and exactly the same
process that was followed when describing the curves of constant
curvature may be as readily applied when the curvature is
constantly varying. In the case of water this distance is,
using the formula already given, $d = \dfrac{ct}{\Delta}$, = ·0128 for cur-
vature $c = 1$, or ·000128 for $c = ·01$. If, therefore, the drop
or other capillary surface is drawn to ten times the true scale,
to effect which the vertical scale of curvature must be mul-
tiplied 100-fold, the dimensions are such as to allow of
considerable accuracy. I am able to show a very beautifully
executed series of capillary curves for alcohol and water
drawn to a scale of 10, which Miss Stevenson has been good
enough to prepare ; but as Prof. Perry has constructed a
large number which are printed in the Royal Institution
Proceedings, to which reference has been made, there is no
occasion to publish them. He, however, would have been
able to draw his in as many hours as he required weeks had
he made use of the method described in this paper.

In order to originate the reciprocal rule I used a screw-
cutting lathe as a dividing-engine, and scratched the wax off
a strip of plate glass with a fine point at the places where
divisions were required. Of course as the distance from ∞
increases and the readings become smaller, the distance
between marks corresponding to a regular series of figures
becomes greater, and new subdivisions are with advantage
added, as is usual where a scale is not one of equal parts.
The glass scale was etched, and the divisions transferred to a
strip of celluloid by means of the well-known method with a

radius-bar or tube to which two needles are fastened. Fig. 3 is a full-size copy of a portion of the glass scale, on which, to avoid fractions, the numbers are all multiplied by 10. The inch unit was chosen instead of the centimetre, as the leading screw had eight threads to the inch.

It is evident that any curves in which simple relations exist between the radius of curvature and the normal, the coordinates, or other easily observed function of the curve, may be readily drawn in the same way, but in general a scale of equal parts is preferable to the reciprocal scale. The gain in accuracy and perfect evenness and smoothness of the curve over what is obtainable by the use of compasses is the same as that which was found in the case of the capillary curves of revolution, but the saving in time will of course, though still considerable, be less. I am able to show as examples the catenary $\rho = -n$, the undulating and the looped elastica $\rho = \pm \dfrac{a^2}{y}$, the particular elastica $\rho = \frac{1}{2}n$, the cycloid $\rho = 2n$ with its identical evolute, the tractrix $\rho = \dfrac{a^2}{n}$ with its evolute the catenary and the evolute of the catenary, the parabola $\rho = 2n$ with its evolute, and all the conic sections (including the parabola) $\rho = \dfrac{n^3}{l^2}$.

It is, moreover, evident that the further use which Lord Kelvin has made of the method of drawing curves from their curvature in order to solve certain dynamical problems is one to which the procedure described in this paper may be applied with advantage.

I must in conclusion express my obligation to Miss Stevenson, who has assisted me in the construction of these rules, and who has spared no pains in setting out the beautifully drawn curves which I am able to exhibit.

DISCUSSION.

Prof. Perry considered the method a new departure of great value. When he (Prof. Perry) drew the capillary surfaces of revolution in 1875, he found that cumulative errors produced considerable discrepancies.

Prof. Henrici thought the method would be a very useful one.

Prof. Greenhill said one would now be able to secure better diagrams of transcendental and other curves than heretofore, and he thought Mr. Boys's method would supplant the laborious processes now used to determine the paths of projectiles. When the resistance varied as the square of the velocity, the elevation for maximum range depended on the initial velocity, and for a cube law, both elevation and range tend to finite limits as the initial velocity increases.

Prof. Minchin inquired whether the catenary could be best drawn by using a scale of equal parts instead of one divided reciprocally.

The President greatly appreciated the saving of labour effected by Mr. Boys's method, and thought the apparatus should be shown at the forthcoming Exhibition of Mathematical Instruments in Germany.

XXI. *A New Photometer.* *By* ALEXANDER P. TROTTER [*].

IN the course of my investigations on the distribution and measurement of illumination [†], which led to the examination of the lighting of several streets and public places in London during the winter of 1891–92, I used illumination-photometers of different kinds. The final form consisted of a horizontal screen of white cardboard, or of paper mounted on glass, having a clear star-shaped hole in the middle. Below this and enclosed in a box was an inclined white screen illuminated by a small glow-lamp. By adjusting the illumination of the lower screen until the star-shaped hole more or less completely disappeared, the illumination of the horizontal screen was measured. After trying several different methods, the illumination of the lower screen was adjusted by altering its inclination to the glow-lamp. I greatly prefer

[*] Read June 9, 1893.
[†] "The Distribution and Measurement of Illumination," Proc. Inst. Civil Engineers, vol. cx. pt. iv. Paper No. 2619.

this arrangement of screens to a Bunsen Photometer, first, because there is only one spot to examine instead of a pair of images, and secondly, because, under favourable conditions, an almost complete disappearance of this spot may be effected, instead of a similarity between two images, as with a Bunsen spot. A Bunsen screen with which the spot disappears on both sides simultaneously is rare. I understand that by warming the grease-spot the edge may be softened, and that simultaneous disappearance may be secured. I have never seen such a screen, and am inclined to think that the disappearance is illusory, and that such softening is probably accompanied by decreased rather than by increased precision.

I have recently applied my arrangement of screens to ordinary light-photometry (as distinguished from illumination-photometry). My first plan consisted of two screens (fig. 1), each inclined at 45° to the direction of the lights and to the eye. One screen was immediately behind the other ; the front screen was perforated, and was mounted on a sliding-carriage on a photometer-bar. The lights were placed, the one a little in front and the other a little behind the plane of intersection of the screens. The back of the perforated screen was blackened and was shaded from the light which illuminated the back screen. The edge of the perforation was bevelled, to assist the complete disappearance of the hole. The hole consisted of two lozenge-shaped apertures one over the other, point to point, the object being to concentrate attention on a vertical line. The screens were held in a frame capable of rotation round a vertical axis through a small angle, for the purpose of producing small and rapid variations. But although one screen thus received more light and the other less, the cosine-law of illumination caused the former to increase but slightly in brightness while the latter diminished considerably. It should be observed that this arrangement of screens, although developed from a Bunsen photometer, turns out to be a modification of the Thompson-Starling photometer, in which two screens at 45° to the lights* and to

Fig. 1.

the eye are used, but side by side, instead of one behind the other.

I showed an extemporized photometer of this kind to Mr. Crompton and to Mr. Swinburne on March 26, 1893. Mr. Crompton suggested the use of zinc for the screens, and this gave me the hint to try perforated zinc, to abandon a definite vertical line of uniform tone, and to select a line or band of uniformity on a screen provided with a number of perforations. I mentioned the idea to Mr. Swinburne on March 27, but had no opportunity of trying it for more than a week. In the meantime Mr. Crompton had thought of a similar expedient, and had used with considerable success a pair of screens, the front one of which was perforated with horizontal slots, and he observed the shading of the slots, say, from bright to dark, and of the bars from dark to bright. Before I had heard of this modification I had tried vertical bars or slots and perforated screens.

The photometer is shown in fig. 2. It consists of a box mounted on a sliding-carriage on a photometer-bar. A slotted screen and a plain screen are fixed inside, and are

Fig. 2.
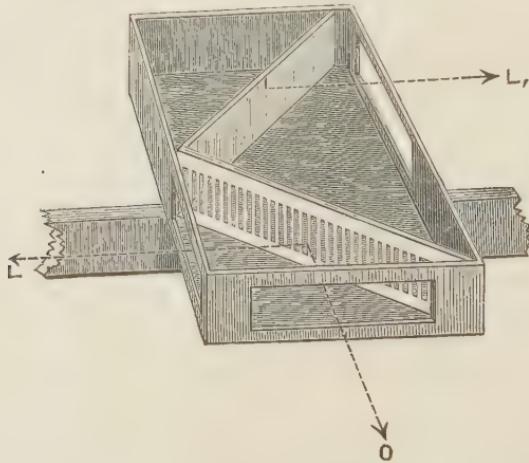
observed through an opening from the direction O. The lights fall on the screens through two openings from the directions L and L_1. The lights are arranged exactly opposite the middle of each side opening, and not in the

plane of the photometer-bar as is usual.   The lower edge of
the front opening or diaphragm carries a sight or pointer,
and the back screen is provided also with a pointer, prefer-
ably at its upper edge.   The photometer may be examined
from a distance, say six or eight feet, and moved by cords.
The two sights should be brought in a line with each other,
and the carriage is to be moved until the band of uniform
tone is bisected by the pointers.   If the carriage be moved
slowly it will be seen that the band of uniform tone remains
fixed relatively to the scale on the photometer-bar.   Readings
might be taken by noting the position of the band over the
scale, but I prefer to follow the usual custom in photometry,
and to keep the scale hidden until an adjustment has been
made, to avoid any bias.   The operation of estimating the
bisection of the band by the pointers is, I think, easier and
therefore more conducive to precision than the estimation of
the similarity of two images as in the Bunsen, or of two
tones as in most other photometers.

    I will now give details of the development of this photo-
meter for the guidance of those who may wish to construct
them.

    Perforated zinc of the ordinary pattern having holes about
0·08 inch diameter and $\frac{1}{8}$ inch pitch, although countersunk
on the back and painted dead white on the front, was found
to be of no use, probably because the area of the holes was
considerably less than that of the remaining metal.   Half-inch
holes punched in thin card at 0·6 inch pitch gave on the whole
a better result than $\frac{1}{4}$-inch or $\frac{1}{8}$-inch wooden slips, bevelled,
glued to a frame at a distance apart equal to their width,
and painted dead white.   I then tried a number of different
patterns of perforated zinc and other screens, 30 centim. long
by 6 centim. high.   Circular perforations of 4 millim. and
6 millim. diameter, about half the metal being removed, were
less satisfactory than zinc with holes 13 millim. diameter,
rather more than half the metal being removed.   A fancy
pattern was tried, but without good results.   The suitability
of the screens was compared in some cases by comparison of
the " mean error of a single observation " calculated in the
ordinary way from ten measurements, two similar glow-lamps
being used, sometimes 2 metres apart, and sometimes 3 metres.

Finding that thinness of edge and perfect flatness were very important, I made several screens of card and of paper stretched while wet on wooden frames. The coarsest of these consisted of strips of two-sheet Bristol board 8 millim. wide and 8 millim apart, 18 such strips, with an 8 millim. space at each end, making 30 centim. The strips were cut with sharp scissors to avoid any burr on the edge. Another screen has strips 5 millim. wide, and a third has 48 strips 3 millim. wide. White foolscap paper was used, soaked in hot water to remove the glaze ; it was mounted whole, and cut on glass when dry, the knife being held slanting as in mount-cutting, to bevel the edge. These screens I find, and my opinion is confirmed by friends who are for the most part unfamiliar with photometers, to be easy to use.

The advantage of vertical strips is that perfect symmetry is attained, a condition which I find to be important in photometry. When perforations are used, dark holes on a light ground are seen at one end of the screen, and light holes on a dark ground are seen at the other end. The balance is indicated by a strip at which these two patterns become confused. I have not succeeded in getting a definite easily-bisected strip of uniform tint with such screens. When, however, strips are used, the spaces being equal to the width of the strips, the appearance of the screen is perfectly symmetrical, and the effect is improved by the use of a diaphragm cutting off from the observer the edges of the screens. When lights of similar colour are compared, the light strips and dark spaces at one end are almost indistinguishable from the dark strips and light spaces at the other end. With 8 c. p. lamps 3 metres apart, and with the 8 millim. strips, one strip or one space can generally be made to disappear ; that is to say, its edges become invisible when examined from a distance of 6 or 8 feet. At a shorter distance, one eye only must be used. With the 3 millim. strips, a nearly uniform grid is seen, paling a little towards the middle, and showing a band about 15 millim. wide, including, say, three strips which are indistinguishable from the two spaces between them. When lights differing in colour are used, the narrow strips are distinctly preferable : disappearance is nowhere complete, but the differently

coloured strips and spaces seem to blend at a definite place on the screen.

As screens 30 centim. long and requiring a box about 50 centim. by 30 might be considered cumbersome, I have made a photometer with screens 22·5 × 3·7 centim. effective ; these are contained in a box measuring about 22 × 32 × 6 centim. deep. The diaphragm facing the observer is 15 × 3·5 centim. The screens are placed at 45° to the lights and to the observer. I have tried other inclinations, but there is no difficulty in getting a surface sufficiently dead-white to work well at this angle.

I find that a distance of 3 metres between two 8 c. p. glow-lamps is rather great for easy reading of the photometer. A distance of 2 metres is perhaps a little too small ; that is, if a greater distance conduces to greater precision, it might be worth while to employ an intermediate distance. Again, a distance of 3 metres seems suitable for comparing a pair of 16 c. p. lamps. This question may be considered from the point of view of a gradient or fall of illumination ; and it is not unlikely that for any one observer, the " mean error of a single observation " is a function of the gradient of illumination at the photometer-screen.

With an 8 c. p. lamp at 1·5 metre, the illumination is 3·56 times a candle-metre; that is, it is 3·56 times the illumination which would be given by one candle at one metre. The gradient is 0·0474 candle-metre per centimetre. With a 2-metre photometer-bar and a pair of 8 c. p. lamps, the illumination due to one lamp is 8 candle-metres, and the gradient is 0·16 candle-metre per centimetre. With a 16 c. p. lamp on a 3-metre bar, the gradient at the middle, $i. e.$ 1·5 metre, is 0·948 candle-metre per centimetre. My experience with the form of photometer described in this paper goes to show that a gradient of less than 0·1 candle-metre per centimetre is not desirable. It should be observed that the illumination on a screen inclined at 45° to the light is about 0·7 of that of a screen turned full to the light.

To graduate a photometer-bar of unit length, and having a unit light at one end, I have used the formula :—

$$l = \frac{1}{1 + \sqrt{n}},$$

where l is a length measured on the bar, and n is proportional to the light to be measured. With a 3-metre bar a displacement of 3·9 millim. from the middle is equivalent to a difference of 1 per cent. in the candle-power of the two lights. Since the band of uniformity with the 3 millim. strips may be said to be about 15 millim. wide, and 15·6 millim. are equivalent to 4 per cent., it is only by bisection of this band that a precise measurement can be made. In using the 8 millim. strips, if the middle of one strip or space is the actual point of balance, and by error the space or strip on one side of it be taken as the one indicating the balance, the error is ± 2 per cent. Out of several series of 12 observations, the mean error of a single observation has frequently fallen below this. Among some results which I consider satisfactory, compared with my experience with other photometers, are mean errors of 0·85 and of 1·48 per cent. calculated from sets of 12 readings.

Note added June 5, 1892.

In further tests with this photometer the mean errors have generally fallen below 1 per cent.

At first I tried other angles than 45°. For whitewashed metal screens this angle appears to be suitable. I find, however, that it is preferable with paper screens to use an angle of incidence of 35°, and I understand that this angle is used in the Thompson-Starling photometer.

When the light to be measured is subject to small variations, the direction and the magnitude of the variations are plainly visible by the movement of the band of uniform tone. This photometer has been chiefly used for such measurements, and seems to be well adapted for them.

XXII. *Some Notes on Photometry.*
By SILVANUS P. THOMPSON, *D.Sc., F.R.S.**

I. *On the use of Two Overlapping Screens as an Isophotal.*

THE employment of two opaque screens placed at an angle
with one another, and with the observer's eye, to receive
the light from the two sources, was made the basis of a
photometric method by the author, in conjunction with Mr.
C. C. Starling, in 1881. Their photometer was designed, in the
first instance, for electric-light measurements. For this special
end any method in which opaque screens are used is to be
preferred, in one respect, to those methods in which trans-
lucent screens are used; for then it is not necessary to employ
coloured glasses when making the comparison, as the opaque
screens themselves may be of coloured material, and the choice
of opaque coloured material is much more varied than is that
of tinted glass.

In the Thompson-Starling photometer the two surfaces for
receiving the light met at an angle of about 70°. The pair
of screens was constituted by two pieces of card, either white
or coloured, or by two surfaces of some brilliantly-tinted
fabric mounted on card, each pair being dropped down into
position over a wedge-block. The observer, placing his eye
opposite the *arête* of the dihedral pair of screens so as to
view each surface at the same angle, had to adjust the appa-
ratus until the apparent illumination at the adjacent parts of
the two surfaces was equal.

When working with this photometer it was found that the
precision of judgment of the eye as to equality of the two

Fig. 1.

illuminations was impaired if by bad workmanship any con-
siderable width of blunted edge intervened between the two

* Read June 9, 1893.

surfaces that should have met with precision. There was a similar defect in the original form of the Bouguer photometer, wherein the opaque partition was continued down to the screen and interposed an unilluminated patch equal in breadth to its own thickness between the two illuminated surfaces. In that case the remedy, applied by Foucault in the modified instrument which bears his name, was to shorten the partition so as to permit the two illuminated parts to come just into optical contact. In the case of the oblique opaque screens the author tried to remedy the defect by several modifications, which, though not described at the time, proved useful. In one of these each surface was extended so as partially to overlap the adjacent surface, as indicated in figs. 2 and 3. In

Fig. 2.

Fig. 3.

PLAN
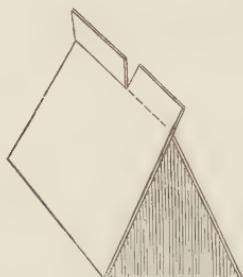
another instrument the overlap was given as shown in figs. 4 and 5.

Fig. 4.

Fig. 5.

PLAN
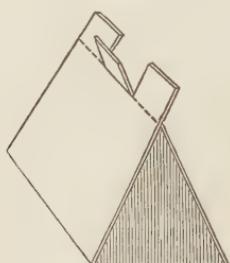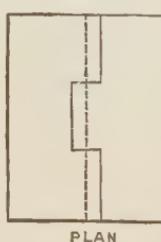
The principle of overlap in photometers operating by the diffuse illumination of opaque inclined screens has since been used by Sir John Conroy*, who employed two pieces of white

* Phil. Mag. 1883, vol. xv. p. 423.

writing-paper, inclined at about 60° to one another. He found, as I did, that 90° is too large a dihedral angle for exact work. Materials such as card and paper are never entirely devoid of specular reflexion. At an incidence of 45° on each surface there is so much specular reflexion of the light as quite to vitiate the observations.

II. *Periodic Principle in Photometry.*

It is a familiar fact to every person who is accustomed to use the Bunsen photometer that the most convenient way of taking observations is to swing the carriage which supports the screen and mirrors backwards and forwards through a continually-decreasing distance about the position of balance. That position is ultimately found by the eye noting the successive departures from equality produced by rapid small displacements on either side of the position of balance. If these inequalities can be made equal *inter se*, the zero will lie midway ; for though the intensity of illumination (assuming the sources of light as points) varies inversely as the square of the distance, the change of intensity for a small change of distance will (irrespective of sign) be the same, to within a small quantity of the next higher order, whether the displacement be toward the standard light or from it. For, taking the initial distance as unity when the illumination is i, then (neglecting small quantities of higher orders), when the distance 1 is made $1 + \delta r$, the proportional change of illumination is $\mp \dfrac{2\delta r}{1 + 2\delta r}$; or, the change of illumination is approximately inversely proportional to the distance, and is approximately proportional directly, but with contrary sign, to the displacement.

It is a consequence of the law of fatigue that the eye is much more sensitive to small differences of illumination when these are successively produced than when they are constant in amount. Hence it is easier to estimate the true zero position on the photometer by these successive movements than by simply trying to put the carriage at zero. A difference due to a displacement of two millimetres of the stationary carriage might fail to be perceived. But the eye

cannot fail to detect the difference when the carriage is quickly swung to and fro over the distance of two millimetres on either side of the zero ; and, judging of the two inequalities on either side, and altering the adjustment until the two inequalities are themselves equal, the zero is determined. As a piece of pure physics, this method of successively approximating to a zero, ill-defined in itself, by means of equal errors on either side, may be compared with the method of Joule for determining the temperature of minimum volume of water.

In the researches of Abney and Festing on the photometry of colour, the same principle of estimating the position of balance by producing rapid movements giving successive inequalities on either side of the final position was used ; and indeed it constitutes the basis of the method of measuring the illuminating effect of any coloured light at different parts of the spectrum. In this case also the motion was produced by hand, by shifting a lever rapidly to and fro.

Indeed it seems probable that, without any specific recognition of its meaning or importance, the method of roughly producing periodic variations in the relative intensities of the two lights has been quite commonly used by many individuals in their work with photometers.

It has seemed to the author worth while to generalize this principle, and to give · definition to it, as a recognized principle in photometry. He has therefore essayed to construct a *Vibration-photometer* in which periodic changes in the illumination are deliberately produced, in order thereby to effect systematically that which has hitherto been done unsystematically and by hand.

There are several ways of arriving at the desired result :—

(1) The " screen " of the photometer may be so mounted on its carriage as itself to vibrate through a small distance, at some convenient frequency.

(2) The standard light may be moved in some periodic way to and fro through a small distance ; though in the case of flame-lamps this is inadvisable.

(3) The light of the standard lamp may be made to vary by a small percentage in a periodic manner; one way being by revolving in front of it a fan with narrow

arms to obscure periodically a portion of its light; another way being to vary periodically the aperture of a Methven slit.

(4) The light that is to be measured may be made to vary periodically by some known fraction of itself in ways analogous to the preceding.

When any of these things are done the position of balance can be found by simple direct adjustment without the usual delays.

The particular form in which the author has worked out the periodic principle is by mounting upon a spring on the photometer-carriage the paraffin-block translucent " screen " of Dr. Jolly. He finds that a period of $\frac{1}{3}$ of a second is convenient for the purpose. He has also tried an eight-bladed fan, with narrow blades to obscure in passing a fraction of the light. In using with either of these periodic devices the paraffin-block apparatus of Jolly, there is found a curious optical illusion which assists the judgment. As the brightness of the illumination of the two adjacent halves of the divided block passes through the state of equality, from being brighter on the one side to being brighter on the other, the narrow dark line which divides the two luminous portions appears itself to shift, the apparent displacement being away from the more luminous side. Whether this optical effect arises from irradiation, or from any other cause, it certainly assists the eye in its judgment as to the position of balance.

Some further observations with this device are still in progress.

III. *The Electric-Arc Standard of Light.*

In 1878 experiments were made at Chatham by Abney, Cardew, and others upon the electric arc, in the course of which the practical invariability of the intrinsic illumination of the crater-surface of the positive carbon of the arc was established. Abney and Festing, in their researches on the photometry of colour, have since that period used as a standard of white light the light of the crater of the arc. As pointed out by the writer some years ago, this invariability of whiteness, which implies invariability of temperature, is necessarily due to the constancy of the temperature of vola-

tilization of carbon. The introduction into the substance of
the carbons of any material having a lower temperature of
volatilization, or of any compound which has a temperature
of dissociation lower than that of the volatilization of carbon,
necessarily lowers the intrinsic brilliancy of the light. This
having once been realized, it seemed only natural to suggest
as a standard of light the light emitted from a given area
of the crater-surface of a pure carbon. This suggestion was
made by the author last year when writing from Rome some
remarks for the discussion of the paper by Mr. Trotter on the
light of the electric arc, read before the Institution of Elec-
trical Engineers. A similar suggestion was independently
made by Mr. Swinburne in the same discussion. For some
time past the author has been considering the experimental
methods for putting into practice the suggestion then made.
The only way to secure a constant effective area of crater
is to produce a much larger crater than is required, and
cover it by an opaque screen pierced with a suitable aperture
of standard dimensions. As this screen must be placed very
near the arc, it must be kept cool artificially by circulation of
water *.

As the intrinsic light of the arc is not far from 70 candles
per square millimetre, a circular aperture 1 millimetre in
diameter will afford a light of about 55 candles. Hence
an aperture smaller than this is preferable in a photometric
standard for all ordinary photometric work. For the purpose
of a special photometric standard with which to compare
other arc lamps, a standard light of 1000 candles or some
such magnitude is doubtless advantageous. This would
require about 14·3 square millimetres of crater-surface, or a
circular aperture of about 4·25 millimetres in diameter. For
ordinary photometric work an aperture of 0·674 millimetre
diameter would give a light of about 25 candles. It is easy
to ensure the illumination of the apparent aperture by employ-
ing a magnet to deflect the arc to the front face of the
carbon.

* M. A. Blondel has recently presented to the Société de Physique
of Paris an apparatus denominated *arc normale,* in which is embodied
this method of carrying out the proposal of Mr. Swinburne and of the
author.

There is an advantage in using as a standard source of light one whose light is greater than that of the light under measurement. The light under measurement may obviously be balanced either against a smaller light brought nearer to the screen, or against a greater light placed at a greater distance. But if the greater light at the greater distance be employed, there will, for any given inequality of illumination on the screen, be required a greater actual displacement of the moving part, whether screen or light, in order to arrive at the position of balance. Hence any error in reading the scale will be a lesser fraction of the quantity to be measured.

It might have been supposed, *à priori*, that for a given total length of photometric bench (as in a Bunsen photometer from lamp to lamp) the position of the photometric screen which would make errors a minimum would be the position in the centre ; balance being sought between two approximately equal lights. Geometrically it is true that a given displacement along the scale produces a minimum change in the difference of the two illuminations, if the point selected be at the centre of the scale. On the other hand, if the total length of the bench be not fixed, it is obvious that the length of bench which will, for a given change in the difference between the two illuminations, yield the greatest actual displacement along the scale will be an infinite length. But in that case, unless infinite lights are used, the two illuminations will both be zero. In fact there is another element to be considered, namely, that in order to make a satisfactory comparison, the eye requires a certain actual brilliancy of illumination in the two surfaces which are to be compared together. Doubtless the habitual patterns of photometer in use have come by natural selection to be of the size that they are in consequence of the absolute magnitudes of the standards habitually employed. If a six-foot bench is the right sort of apparatus to satisfy the needs of the eye when the standard is of one or two candle-power, obviously a longer bench will be right when standards of greater power are employed, as giving, on the average, similar absolute illuminations of the working surfaces. If the degree of accuracy implied in the possibility of reading to within 2 millimetres' length of scale can be attained with a Bunsen or Jolly photometer when the

standard is a 2-candle Methven slit, or a pair of standard
sperm-candles, an accuracy of five times as great ought to be
attainable if there is used as a standard a 50-candle light.
The bench need not in this case be five times as long as the
ordinary bench : the half length only need be elongated five-
fold on one side of the screen, the other half remaining as
before.   In other words, if the standard is of 25-fold brilliancy,
it must be placed 5 times as far away from the screen as
before in order to balance a given light at the same distance
as before on the other side.   And, so far as such standard
light is concerned, a five-fold accuracy will be attained, since
any error or uncertainty in reading the scale will be now but
a fifth part of the whole scale-reading.

All the foregoing points, then, to the use of a brighter
standard light and a longer photometric bench than hereto-
fore.   Such a standard might well be afforded by an arc
crater viewed through a circular aperture 1 millimetre in
diameter, giving about $0·7854$ sq. millimetre of crater-surface,
with a light of about 55 candles.   This should be placed at
one end of a bench some five metres in length ; the gradua-
tions of the scale being, of course, reckoned from the edge of
the aperture *.

One not unimportant advantage of the use of such a pin-
hole standard is that it may with real propriety be treated as
a luminous point ; whereas no one can maintain that the
flame standards habitually used are even approximately points
relatively to the distances at which they are set from the
screen.   For these the law of inverse squares cannot possibly
be true ; though it is never the practice to make any correc-
tions for the errors arising from the size of the flame.

In contemplating the use of circular apertures pierced in
metal diaphragms, it becomes necessary to inquire how far
the thickness of the metal will interfere with the illumination
of the screen in directions not absolutely in line with the axis
of the aperture.

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* It is curious to note in this respect that there is usually an erroneous
instruction observed with the use of the Methven slit ; the distance of the
photometric screen being reckoned from the flame behind the slit instead
of from the slit itself.

Let the screen PQ (fig. 6) be at a distance $x = OX$ from the diaphragm at O ; the thickness of the diaphragm being called δx, and the radius of the circular aperture in it a. It is required to find the illumination at a point P on the screen

Fig. 6.
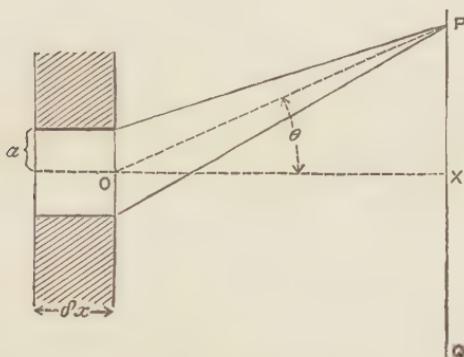
at a distance $PX = y$ from the point X which is on the axis of the aperture. Let the angle POX be called θ. Then the apparent aperture as viewed from P will be bounded by two portions of ellipses (the front and back edges of the hole viewed in perspective), having each as semi-major axis a, and as semi-minor axis $a \cos \theta$. These two ellipses will overlap, their centres being displaced by an amount equal to $\delta x \sin \theta$. If we represent the ratio of the thickness of the metal to the radius of the aperture as $\delta x/a = \tan \phi$, then we may write the following expression for A, the effective area of the hole, as visible in the direction OP, as follows :—

$$A = 2a^2 \left\{ \cos \theta \cdot \left[\frac{\pi}{2} - \sin^{-1}(\tan \phi \cdot \tan \theta) \right] - \tan \phi \cdot \tan \theta \sqrt{1 - \tan^2 \phi \cdot \tan^2 \theta} \right\}.$$

Hence, since the real hole has area $= a^2 \pi$, the ratio η of the illumination at P to the central illumination at X will be

$$\eta = \frac{2}{\pi} \left\{ \cos \theta \cdot \left[\frac{\pi}{2} - \sin^{-1}(\tan \phi \cdot \tan \theta) \right] - \tan \phi \cdot \tan \theta \sqrt{1 - \tan^2 \phi \cdot \tan^2 \theta} \right\}.$$

Case (i). To get some idea of the magnitudes involved, let us take a concrete case. Suppose the aperture to be a hole 1 millimetre in diameter and the diaphragm $\frac{1}{2}$ millimetre thick; so that $\tan \phi = 1$. What will the ratio be of the oblique to the central illumination for a point P 2·5 centimetres from the centre X of a screen which is itself at a distance OX = 50 centimetres from the aperture? As a Bunsen disk is seldom more than 5 centimetres in diameter, this is rather an extreme case. Here $\tan \theta = 2\cdot5 \div 50 = 0\cdot05$. So $\theta = 2° 52'$, and $\cos \theta = 0\cdot9975$. Whence $\eta = 0\cdot935$.

Case (ii). Suppose the hole in the diaphragm to be still 1 millimetre in diameter, but the diaphragm to be 2·5 millimetres thick. Here $\tan \phi = 5$. Taking θ as before, we get $\eta = 0\cdot684$.

Case (iii). Suppose, on the other hand, that the hole is made in metal foil only 0·1 millimetre thick, or that, having been pierced through thicker metal the edge has been cut away by countersinking, leaving only a narrow rim 0·1 millimetre in thickness. In this case $\tan \phi = 0\cdot2$. In the case where thin foil is used there is a considerable risk of the metal buckling with heat : and it would be quite possible, from this cause alone, that the axis of the aperture should become oblique by as much as 10° from the axis of the photometer. Assuming then $\theta = 10°$, the ratio of the central illumination, as thus perturbed by the error of centering, to its unperturbed value will be $\eta = 0\cdot940$.

From all this it may be concluded that unless due care be taken in the selection and proper centering of the diaphragm, errors of several per cent. may arise in the photometric measures executed with its means. The errors arising from thickness of the diaphragm diminish, whilst those arising from defect of centering are increased, as the distance from the photometer-screen to the aperture is increased. In the above investigation no account has been taken of the effects of diffraction, which in the case of very minute apertures might become important.

DISCUSSION.

Major-General Festing, in opening the discussion on both papers, said reflection from the sides of the hole in a thick plate would tend to lessen the error calculated by Prof. Thompson.   The ordinary impurities in carbon were not likely to alter the brilliancy of the crater.   Capt. Abney and himself had no reason to distrust its constancy.   Both the vibrating photometer and Mr. Trotter's arrangement would be very useful.

Dr. Sumpner said his photometric experience had been obtained with the Bunsen, Jolly, and Lummer-Brodhun types. With the two former the inaccuracy arising from uncertainty of adjustment was about $\frac{1}{2}$ per cent. ; changes of about 0·4 per cent. (average) resulted from reversing the screens.   The Lummer-Brodhun instrument (which he described) was better than either of the other two, the average error being about $\frac{1}{4}$ per cent.

Mr. Frank Wright thought scientific men gave too little attention to the question of light standards.   Photometers could be relied on much more than any standard at present in use.   The Methven screen was the most practical standard yet devised, but in his opinion no gaseous flame could be a real standard on account of the influence of the surrounding atmosphere.

Prof. Ayrton saw difficulties in using long benches as suggested by Dr. Thompson, on account of the serious atmospheric absorption which occurs with light from arcs. Decreasing the intensity by dispersion or otherwise was preferable.   In some tests on glow-lamps now being carried on at the Central Institution, a Bernstein lamp used as a standard was mounted on a spring and vibrated.

Mr. Medley showed the vibrating standard referred to by Prof. Ayrton, and gave a series of numbers showing that with this device in conjunction with the Lummer-Brodhun photometer accuracies of about $\frac{1}{5}$ per cent. were obtainable.

Mr. Swinburne thought Mr. Trotter's arrangement was better than the " wobbling " photometer.   As to the best length of bench he was inclined to think the shorter the

better, provided its dimensions were large compared with those of the standard light. He concurred with Mr. Wright in his remarks about the desirability of obtaining a better standard. Speaking of the arc as a standard, he said that only impurities less volatile than carbon would influence the brightness. An important factor was the emissivity of the carbon, which might not be constant.

Mr. Blakesley thought the accuracy obtainable with Mr. Trotter's photometer had been underrated, and pointed out that by using quadrant-shaped screens intersecting orthogonally on the axis of the photometer instead of straight ones, the width of the neutral band could be greatly diminished.

Mr. Trotter, referring to Dr. Thompson's paper, said he had found considerable difficulty in making pin-holes suitable for arc standards. It was not an easy matter to accurately measure the hole when made. In photometric measurements he had found it very important to reverse his screens. Curved screens, as suggested by Mr. Blakesley, had been tried, but with little advantage. They also destroyed the approximate direct-reading property of the photometer. The subject of changing the length of a bench and its effect on the gradient of illumination was discussed. With short benches one had to guard against the departure from the inverse-square law, due to appreciable size of the standard. Recent experiments had shown that the light given out by 1 square millim. of crater-surface differed considerably from 70 candles.

XXIII. *On a new Volumenometer.*
By J. E. MYERS, *B.Sc.*[*]

THE instrument described below was devised by Professor Stroud some three years ago, with the view of having a volumenometer which should be capable of yielding more accurate results than could be obtained by the older methods. After the instrument was made some preliminary trials

showed that considerable difficulties would be encountered in making the instrument air-tight, and in consequence of the pressure of other work the instrument was put on one side till last October, when it was placed in my hands.

Various instruments, the principle of whose action is based on Boyle's law, have been devised with the object of determining the volumes of bodies without immersing them in liquids. The ordinary volumenometer devised by Say is described in textbooks, but a little calculation shows that no great accuracy can be obtained by its use. In Rüdorff's volumenometer* determinations are made by running out mercury from the cavity of the instrument, thereby increasing the volume until the pressure is reduced to a certain fixed value. After the body has been placed in the cavity of the instrument a precisely similar experiment is performed. From a knowledge of the weights of mercury which run out in the two cases, the volume of the body introduced follows by a simple calculation. In Paalzow's volumenometer† measurements are made by increasing the volume of the instrument to a certain fixed limit, first without and then with the body in the cavity. The pressure, recorded by the manometer attached, is noticed in each case, whence the volume required is calculated. Baumhauer's volumenometer‡ differs only from that due to Paalzow in a detail of construction. Messrs. Gee and Harden§ suggest a method of determining the volumes of bodies, depending on the gravimetrical estimation of the carbon dioxide occupying a vessel of known volume in which the body whose volume is required is placed. However, since a cubic centimetre of carbon-dioxide gas weighs only ·0020 gramme, it is clear that the accuracy attainable in determining small volumes by this method cannot be very great. The volumenometer about to be described has proved itself capable of determining the volumes of bodies with as great expedition and much greater accuracy than any of the instruments above noticed.

* Wied. *Ann.* vi. p. 288.   † *Ibid.* xiii. p. 332.

‡ *Archives Néerlandaises.*

§ British Association Report, Newcastle, 1889.

Let A, B, C, D represent the volumes of the cavities of the vessels A, B, C, D.   A is in connexion with C, and B with D, as indicated in the accompanying figure.   Suppose mercury stands at the same level in each of the tubes c and d, and let
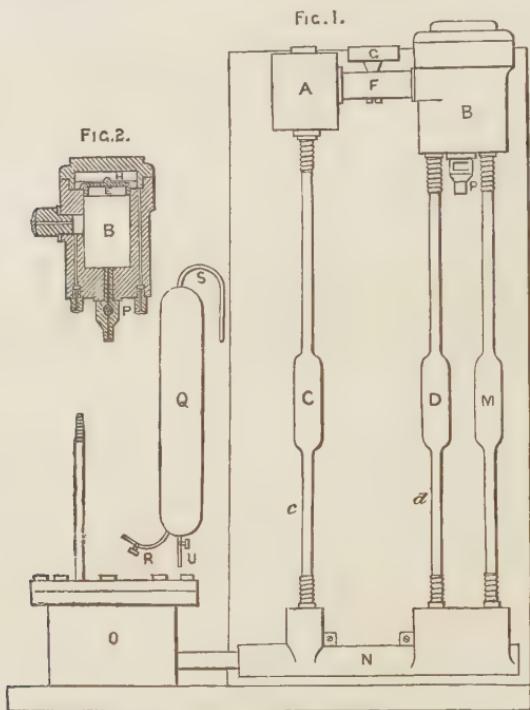
the same pressure applied simultaneously compress volume C into A, and volume D into B.   The condition that the mercury shall still be at the same level in both tubes is that the volume-ratios $\frac{A}{B}$ and $\frac{C}{D}$ shall be equal.   This principle forms the basis of action of the instrument.   The vessel B contains mercury, the quantity of which may be reduced by opening the tap P, and allowing some to run out.   In this way the equality of volume-ratios may readily be obtained.   The two volumes A and B can be put in communication with each other by means of the iron tube F, which is of small bore and is provided with a tap, G.   The main difficulty encountered in

using each of the instruments above described arises from the necessity of hermetically closing the cavity of the instrument. This difficulty we have successfully surmounted by providing that the pressures on the under and upper surfaces of the cap employed to close the instrument shall be approximately equal. The device employed will be readily understood by reference to figs. 1 and 2. C, D, and M are glass pipettes, each of about 20 cub. centim. capacity. They differ from ordinary pipettes in one particular, the upper attachments of the bulbs consisting of glass tubes of capillary bore (1 millim. diameter). The top of the pipette C communicates with the cavity of A. By means of the screw-cap E it will be seen that the cavity of B is divided into two compartments, an upper and a lower. The pipette D communicates with the lower, and M with the upper compartment of the cavity of B. It will be clear from this, that as pressure is applied to the mercury in the reservoir it will force the mercury up the pipettes, thus compressing the air both above and below the screw-cap. An approximate balance of pressure is sufficient to guard against leakage.

The lower extremities of the pipettes are connected to an iron portion N, which is in communication with the reservoir O, containing mercury, to which the arrangement for applying pressure Q, is attached.

At the places of juncture of the pipettes and the iron portions, iron pieces whose diameter is equal to the diameter of the pipettes are screwed in. The joints are made air-tight by means of thick-walled indiarubber tubing carefully wired down. The arrangement Q employed for applying, varying, or removing pressure consists of a cylindrical tube closed at both ends, connected with the water-main by means of a narrow tube R, provided with a tap. The tube S serves to effect communication with the reservoir O, and the pressure may be reduced at any time by opening the tap on the tube U. The pipettes C and D with the connecting piece N form a convenient differential manometer.

In making determinations with this instrument, an excess of mercury is placed in the lower compartment of the vessel B. The caps E and H are screwed down to fixed marks, and

pressure is then applied. Owing to the excess of mercury in B the volume-ratio $\frac{A}{C}$ will be greater than the ratio $\frac{B}{D}$. In order to equalize these ratios mercury is run out by opening the tap P. Repeated trials must be made by running out small quantities, removing and then reapplying the pressure after the vessels A and B have been put in communication with each other and with the external atmosphere. The adjustment is complete when the mercury stands at the same horizontal level in both limbs of the manometer, after application of the pressure employed. The instrument is now ready for use.

It is of importance that the pressures in the vessels A and B shall be identical at the commencement of each experiment. This equalization is effected by means of the tap G. The body whose volume is required is placed inside the cavity, the screw-cap replaced, and pressure applied to such an extent that the compression is the same as in the preliminary adjustment, as indicated by the level of mercury in the tube c. Owing to the diminished volume due to the introduction of the body, a further quantity of mercury must be run out in order that the manometer may not indicate any difference of pressure. The quantity which runs out is collected and carefully weighed. Calculation shows that the product of the weight multiplied by the constant of the instrument gives the volume required. For let v represent the volume of B, and let the total internal volume of B and D after the preliminary adjustment has been made $= (n+1)v$. If x is the volume of the body introduced, then the volume originally $(n+1)v$ becomes after compression $\frac{(n+1)v-x}{n+1}$. But if the volume of mercury withdrawn $= dv$, the same final volume is $v - x + dv$. Equating these expressions, we obtain the simple relation

$$x = \frac{n+1}{n}dv = k \cdot dv,$$

where k is a constant, provided the same compression is employed in all experiments. The constant k may be readily determined by measuring with the instrument the volume of a known weight of mercury.

It is convenient therefore to start with the same initial pressure in all experiments. The initial pressure employed is that of the atmosphere, and in order to ensure that such may invariably be the case small holes have been drilled through each of the screw-caps E and H, which holes are perfectly closed by means of screws (not shown in the diagram) before the application of pressure in each experiment. The internal volumes of A and B are each approximately 16 cub. centim., and the capacities of C and D as above stated 20 cub. centim.

It is difficult to compare the instrument with those due to Paalzow and Rüdorff under similar conditions. In the present instrument air under about two atmospheres' pressure is employed, while in previous instruments rarefaction has always been resorted to. This alone suffices to produce increased accuracy. Paalzow shows, in the paper above referred to, that with his instrument a difference of pressure of $0 \cdot 1$ millim. involves a change of volume $= 0 \cdot 016$ cub. centim. In Professor Stroud's instrument the same pressure-difference requires a change of volume $= 0 \cdot 0023$ cub. centim.,—that is, the accuracy is 7 times as great. Rüdorff does not give the dimensions of his instrument, so that comparison cannot be made in this manner. From a series of results which he gives, however, the calculated mean error $= 0 \cdot 008$ cub. centim., and this is much larger than the error deduced from any of the series of results given below.

The following are specimens of some of the earlier results which have been obtained. At Prof. Stroud's suggestion the first uncertain figure (or estimation figure) in the result is indicated in small type :—

I. Determination of the Volume of an Iron Cylinder.

$v = 3 \cdot 97_6$.
$\quad 3 \cdot 97_1$.
$\quad 3 \cdot 97_8$.   Volume deduced from measurement $= 3 \cdot 97_7$.
$\quad 3 \cdot 96_8$.

II. Determination of Specific Gravity of $CuSO_4\ 5H_2O$.
Weight of $CuSO_4$ employed was about 6 grammes.
The following are the results of successive determinations :—

S.G. $= 2\cdot28_0$.   Volume $= 2\cdot64_0$ cub. centim.

    $2\cdot28_8$. $2\cdot63_0$.

    $2\cdot28_4$. $2\cdot63_5$.

    $2\cdot28_5$. $2\cdot63_3$.   Mean error $= \cdot003$ cub.

    $2\cdot28_6$. $2\cdot63_2$.   centim.

    $2\cdot28_4$. $2\cdot63_5$.

    $2\cdot28_0$. $2\cdot64_0$.

III. Determination of Specific Gravity of a piece of Cork. The volume of cork employed was about 10 cub. centim.

S.G. $= \cdot172_1$.   Volume $= 10\cdot17_9$ cub. centim.

    $\cdot172_2$. $10\cdot17_3$.

    $\cdot172_1$. $10\cdot17_9$.   Mean error $= \cdot002$ cub.

    $\cdot172_3$. $10\cdot17_1$.   centim.

IV. Determination of Specific Gravity of very finely divided Cork Dust.

A value greater than the one above given for cork was of course expected. The cork dust was contained in a small cylindrical glass vessel of about 3 cub. centim. capacity and almost filled it. The experiments prove, however, that the volume actually occupied by the cork was only about 0·569 cub. centim.

S.G. $= \cdot95_6$.   Volume $= \cdot56_9$ cub. centim.

    $\cdot95_4$. $\cdot57_1$.

    $\cdot94_9$. $\cdot57_4$.   Mean error $= \cdot002$ cub.

    $\cdot96_2$. $\cdot56_6$.   centim.

    $\cdot95_9$. $\cdot56_8$.

    $\cdot95_4$. $\cdot57_1$.

A slight difficulty occurs when one is letting out mercury from the vessel by opening the tap, in consequence of the spasmodic change in level of the mercury in the capillary tube. A more accurate method of procedure consists in having the capillary tube graduated, and observing as follows :—Read the position of the mercury column when it is just above or below the standard position, then open the tap to permit a small drop of mercury to escape, which is separately collected and weighed, so that, after reading the position of the mercury column, a simple linear interpolation suffices to calculate the exact quantity of mercury which would have escaped if the equilibrium had been exact.

The following are successive determinations of the volume of an iron cylinder, employing this mode of procedure :—

Volume $= 6 \cdot 50_7$ cub. centim.

$6 \cdot 50_8$

$6 \cdot 50_9$.

$6 \cdot 50_8$.

$6 \cdot 51_0$.   Mean error $= \cdot 0009$ cub. centim.

$6 \cdot 50_9$.

$6 \cdot 50_7$.

$6 \cdot 50_9$.   Volume of cylinder deduced from

$6 \cdot 50_8$.   measurement $= 6 \cdot 50_9$ cub.

$6 \cdot 51_0$.   centim.

Mean $= 6 \cdot 50_{85}$.

The instrument would probably be very suitable for the accurate determination of the specific gravity of samples of gunpowder.

XXIV. *The Magnetic Field close to the Surface of a Wire conveying an Electrical Current.* *By* Professor G. M. MINCHIN, *M.A.*[*]

AT a meeting of the Physical Society on March 10, 1893[†], I gave an expression for the conical ("solid") angle subtended at any point, P, in space by a circle occupying any position.

When the circle is the seat of an electric current, this conical angle is the measure of the magnetic potential produced at P by the current. The conical angle is usually expressed by a series of spherical harmonics, or, rather, by two such series, one of which is to be used when the distance of P from the centre of the circle is less than the radius of the circle, and the other when it is greater. The expression which I have given consists of two complete elliptic integrals of the third kind, which for convenience I reproduce here.

[*] Read June 9, 1893.
[†] *Ante*, p. 204.

In fig. 1 let A C B be the circular current; P′ any point in space at which the value of the conical angle subtended by the current is Ω ; O V the central axis perpendicular to the

Fig. 1.
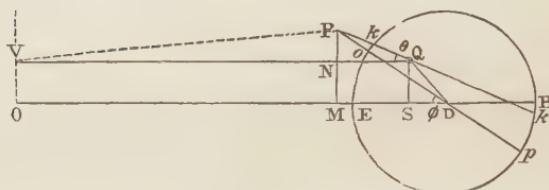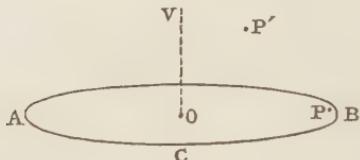
Fig. 2.

plane of the current ; A B the diameter of the current determined by the plane P′ O V; ν the sine of the angle P′ O V; $\rho = $ A P′, $\rho' = $ B P′; z the perpendicular from P′ on A B. Then, a being the radius O B, and O P′ $= r$, we have

$$\Omega = 2\pi - \frac{2z}{\rho r}\left\{ \frac{r-a}{1+\nu}\Pi\left(\frac{-2\nu}{1+\nu}, k\right) + \frac{r+a}{1-\nu}\Pi\left(\frac{2\nu}{1-\nu}, k\right)\right\}, \quad (1)$$

where $k^2 = 1 - \dfrac{\rho'^2}{\rho^2}$, and Π is the symbol for the complete elliptic integral of the third kind, its parameter and modulus, respectively, being the quantities included in the brackets following Π, according to the ordinary notation of such integrals, viz.,

$$\Pi(n, k) = \int_0^{\pi/2} \frac{d\omega}{(1 + n\sin^2\omega)\sqrt{1 - k^2\sin^2\omega}}.$$

If P′ lies anywhere in the plane of the circle and *inside* its area, $\Omega = 2\pi$; and if P′ lies anywhere in the plane of the circle and *outside* its area, $\Omega = 0$. If P′ is taken strictly *on* the circumference of the circle, and if the circle is a strictly Euclidian curve, *i. e.* something absolutely devoid of breadth,

Ω is necessarily indeterminate. It is well known that all the surfaces of constant conical angle subtended by a circuit of any form, plane or tortuous, contain the circuit as a bounding edge, and that any two surfaces for which $\Omega = \Omega_1$ and $\Omega = \Omega_2$ are inclined to each other at the constant angle whose circular measure is $\frac{1}{2}(\Omega_1 - \Omega_2)$ at all points on this common bounding edge.

When the circuit is not a Euclidian curve, but a wire, and the point in space which we consider is near the surface of the wire, as at P in fig. 1, it becomes necessary to take account of the dimension of the cross-section of the wire, and the conical angle subtended at P is the integrated result of dividing the normal cross-section made by a plane through P into an indefinitely great number of indefinitely small elements of area and breaking up the wire into a corresponding number of circular filaments having these elements of area for cross-sections, these circular filaments having all the same central axis, OV, and their planes being, of course, all parallel.

Our object now is to find the value of Ω when P is very close to the wire both when the current is assumed to be of constant density and when it is assumed to be of variable density in the cross-section. Fig. 2 represents the normal cross-section of the wire (supposed to be a circle) made by a plane through P.

Take any point, Q, in the cross-section, and at Q take an indefinitely small element of area, dS. If i is the total current flowing through the cross-section of the wire, the current in the first case through dS is $i \dfrac{dS}{\pi c^2}$, where c is the radius of the cross-section; and if Ω is the conical angle subtended at P by the circular filament of the wire passing through dS, the magnetic potential at P due to the current in the filament is

$$i\Omega \frac{dS}{\pi c^2}. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (2)$$

Let the radius, QV, of the filament be denoted by α; let $PQ = R$; $\angle PQV = \theta$; then in the typical formula (1) we have $\rho' = R$, $r = VP$, $z = PN = R \sin \theta$, R being, of course, very small compared with α.

Although our object is to obtain the value of Ω correctly to the *second* order of the small quantity $\dfrac{R}{\alpha}$, it will be useful for future reference to express the quantities ν, &c. as far as the third order.

Thus we have

$$r = \alpha\left(1 - \frac{R}{\alpha}\cos\theta + \frac{R^2}{2\alpha^2}\sin^2\theta + \frac{R^3}{2\alpha^3}\cos\theta\sin^2\theta\right), \quad . \quad . \quad (3)$$

$$\frac{1}{\rho} = \frac{1}{2\alpha}\left\{1 + \frac{R}{2\alpha}\cos\theta + \frac{R^2}{8\alpha^2}(3\cos^2\theta-1) + \frac{R^3}{16\alpha^3}(5\cos^3\theta - 3\cos\theta)\right\}, \quad (4$$

$$\frac{1}{r} = \frac{1}{\alpha}\left\{1 + \frac{R}{\alpha}\cos\theta + \frac{R^2}{2\alpha^2}(3\cos^2\theta-1) + \frac{R^3}{2\alpha^3}(5\cos^3\theta - 3\cos\theta)\right\}, \quad (5)$$

$$\nu = \frac{R\sin\theta}{r}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

$$k' = \frac{R}{\rho}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

where k' is the modulus complementary to k.

Now, of the two elliptic integrals in (1) the first is one in which the parameter is negative and nearly equal to -1, while in the second the parameter is positive and very large.

Observe that $\dfrac{\tilde{z}}{r} = \sin\text{PVQ} = \sqrt{1-\nu^2}$, so that (1) can be written

$$\Omega = 2\pi - \frac{2}{\rho}\left\{(r-\alpha)\sqrt{\frac{1-\nu}{1+\nu}}\,\Pi\left(\frac{-2\nu}{1+\nu}, k\right)\right.$$
$$\left. + (r+\alpha)\sqrt{\frac{1+\nu}{1-\nu}}\,\Pi\left(\frac{2\nu}{1-\nu}, k\right)\right\}. \quad (8)$$

To deal with the first of these elliptic intervals, let

$$\frac{2\nu}{1+\nu} = 1 - k'^2\sin^2\psi, \quad \therefore \sqrt{\frac{1-\nu}{1+\nu}} = k'\sin\psi\,; \quad . \quad (9)$$

and for the second, let

$$\frac{2\nu}{1-\nu} = \cot^2\chi, \quad \therefore \sqrt{\frac{1+\nu}{1-\nu}} = \frac{1}{\sin\chi}. \quad . \quad . \quad (10)$$

Hence we have

$$\Omega = 2\pi - 2\left\{ \frac{r-\alpha}{\rho}\,.\,k'\sin\psi\,\Pi(-1+k'^2\sin^2\psi,\,k)\right.$$

$$\left. + \frac{r+\alpha}{\rho}\,.\,\frac{1}{\sin\chi}\,\Pi\,(\cot^2\chi,\,k)\right\}. \quad . \quad (11)$$

The values of these complete elliptic integrals of the third kind are well known in terms of integrals of the first and second kinds. Thus (see Hymers's 'Integral Calculus,' section ix., or any treatise on Elliptic Functions) we have the equation

$$\frac{k'^2\sin\psi\cos\psi}{\Delta(k',\psi)}\{\Pi(-1+k'^2\sin^2\psi,\,k)-K\}$$

$$= \frac{\pi}{2}-K\,.\,E(k',\psi)+(K-E)\,.\,K(k',\psi). \quad . \quad (12)$$

Now

$$E(k',\psi)=E'-\int_\psi^{\pi/2}\sqrt{1-k'^2\sin^2\psi}\,.\,d\psi,$$

and

$$K(k',\psi)=K'-\int_\psi^{\pi/2}\frac{d\psi}{\sqrt{1-k'^2\sin^2\psi}},$$

where E' and K' are the *complete* integrals with modulus k'. Also it is well known that $KE'+K'E-KK'-\frac{\pi}{2}=0$. Hence the right-hand side of (12) becomes, by expanding and neglecting powers of k' beyond the second,

$$E\left(\frac{\pi}{2}-\psi\right)-\frac{k'^2}{4}\left(\frac{\pi}{2}-\psi+\sin\psi\cos\psi\right)(2K-E);$$

so that

$$k'^2\sin\psi\,\Pi(-1+k'^2\sin^2\psi,\,k)$$

$$=k'^2\sin\psi\,.\,K+E\frac{\frac{\pi}{2}-\psi}{\cos\psi}-\frac{k'^2}{4}\left\{(2K-E)\sin\psi+\frac{\frac{\pi}{2}-\psi}{\cos\psi}(2K-E\cos2\psi)\right\}$$

$$=E\frac{\frac{\pi}{2}-\psi}{\cos\psi}+\frac{k'^2}{4}\left\{(2K+E)\sin\psi-\frac{\frac{\pi}{2}-\psi}{\cos\psi}(2K-E\cos2\psi)\right\}. \quad (13)$$

Now since $k' = \dfrac{R}{\rho}$, the first term in brackets in (11) can be written

$$\frac{r-\alpha}{R} \cdot k'^2 \sin \psi \, \Pi(-1 + k'^2 \sin^2 \psi, k),$$

and we have

$$\Omega = 2\pi - 2 \left\{ \frac{r-\alpha}{R} \cdot k'^2 \sin \psi \, \Pi(-1 + k'^2 \sin^2 \psi, k) \right.$$
$$\left. + \frac{r+\alpha}{\rho} \cdot \frac{1}{\sin \chi} \Pi(\cot^2 \chi, k) \right\}, \quad . \quad (14)$$

in which the value of the first elliptic integral is to be substituted from (13).

Dealing now with the second elliptic integral, its value is given by the known equation

$$\frac{\Delta(k', \chi)}{\sin \chi \cos \chi} \Pi(\cot^2 \chi, k) = \frac{\pi}{2} + K \{\tan \chi \Delta(k', \chi) - E(k', \chi)\}$$
$$+ (K - E) \cdot K(k', \chi). \quad . \quad (15)$$

But observe that

$$\sin \chi = \frac{R}{2\alpha} \sin \theta \left(1 + \frac{R}{\alpha} \cos \theta\right),$$

so that χ is a small quantity of the same order as k'. Hence, if in the coefficient of K we neglect quantities of the order $\dfrac{R^3}{\alpha^3}$,

$$\frac{1}{\sin \chi} \Pi(\cot^2 \chi, k)$$

$$= \cos \chi \left\{ \frac{\pi}{2} + (K - E) \cdot \chi \right\}$$

$$= \left(1 - \frac{R^2}{8\alpha^2} \sin^2 \theta\right) \left[\frac{\pi}{2} + \frac{R}{2\alpha} \sin \theta \left(1 + \frac{R}{\alpha} \cos \theta\right)(K - E)\right]. \quad (16)$$

The coefficient $\dfrac{r+\alpha}{\rho}$ to the second order is $1 + \dfrac{R^2}{8\alpha^2} \sin^2 \theta$, so that we have

$$\frac{r+\alpha}{\rho} \cdot \frac{1}{\sin \chi} \Pi(\cot^2 \chi, k) = \frac{\pi}{2} + \frac{R}{2\alpha} \sin \theta \left(1 + \frac{R}{\alpha} \cos \theta\right)(K - E). \quad (17)$$

Hence

$$\Omega = 2\pi - 2 \left\{ \frac{r-\alpha}{R} \left[E \frac{\frac{\pi}{2} - \psi}{\cos \psi} + \frac{k'^2}{4} \left\{ (2K + E) \sin \psi \right. \right. \right.$$

$$\left. \left. \left. - \frac{\frac{\pi}{2} - \psi}{\cos \psi} (2K - E \cos 2\psi) \right\} \right] + \frac{\pi}{2} + \frac{R}{2\alpha} \sin \theta \left(1 + \frac{R}{\alpha} \cos \theta\right)(K - E) \right\}. \quad (18)$$

In the small term we can put $k'^2 = \dfrac{R^2}{4\alpha^2}$, $\dfrac{r-\alpha}{R} = -\cos\theta$, and $\psi = \theta$, so that, neglecting quantities of the order $\dfrac{R^3}{\alpha^3}$,

$$\Omega = 2\pi - 2\left\{ \frac{r-\alpha}{R}\, E\, \frac{\frac{\pi}{2}-\psi}{\cos\psi} + \frac{\pi}{2} + \frac{R}{2\alpha}(K-E)\sin\theta \right.$$
$$\left. + \frac{R^2}{16\alpha^2}\Big[3(2K-3E)\sin\theta\cos\theta + \Big(\frac{\pi}{2}-\theta\Big)(2K-E\cos 2\theta)\Big] \right\}. \quad (19)$$

The relation between ψ and θ is given by the equation

$$k'\sin\psi = \sqrt{\frac{1-\nu}{1+\nu}} = \tan\frac{\lambda}{2}, \text{ where } \lambda = \angle\,\text{PVQ},$$

$$\therefore\ \sin\psi = \frac{\rho}{R}\tan\frac{\lambda}{2},$$

$$\therefore\ \sin\psi = \sin\theta\left\{ 1 + \frac{R}{2\alpha}\cos\theta + \frac{R^2}{8\alpha^2}(5\cos^2\theta - 1) \right\}, \quad . \quad (20)$$

as far as the second order of small quantities; and the series for $\sin^{-1}x$ in terms of x, or rather for x in terms of $\sin^{-1}x$, gives, to the same order,

$$\psi = \theta + \frac{R}{2\alpha}\sin\theta + \frac{R^2}{2\alpha^2}\sin\theta\cos\theta, \quad . \quad . \quad . \quad (21)$$

$$\therefore\ \cos\psi = \cos\theta - \frac{R}{2\alpha}\sin^2\theta - \frac{5R^2}{8\alpha^2}\cos\theta\sin^2\theta. \quad . \quad (22)$$

Also

$$\frac{r-\alpha}{R} = -\cos\theta + \frac{R}{2\alpha}\sin^2\theta + \frac{R^2}{2\alpha^2}\cos\theta\sin^2\theta; \quad (23)$$

so that (19) becomes expressed entirely in terms of θ. Thus,

$$\frac{r-\alpha}{R} = -\left(\cos\psi + \frac{R^2}{8\alpha^2}\cos\theta\sin^2\theta\right), \text{ therefore}$$

$$\frac{r-\alpha}{R}\cdot\frac{1}{\cos\psi} = -\left(1 + \frac{R^2}{8\alpha^2}\sin^2\theta\right),$$

since θ may be put for ψ in the term of the second order; and thus we have now the equation

$$\Omega = 2\pi - 2\left\{ (1-E)\frac{\pi}{2} + E\theta + \frac{R}{2\alpha}K\sin\theta \right.$$
$$\left. + \frac{R^2}{16\alpha^2}\Big[(2K-E)\Big(\frac{\pi}{2}-\theta\Big) + (6K-E)\sin\theta\cos\theta\Big] \right\}, \quad . \quad (24)$$

which is, however, not yet in its simplest form.

Now it is a known result that when k' is small,

$$K = \log \frac{4}{k'} + \frac{k'^2}{4}\left(\log \frac{4}{k'} - 1\right), \quad . \quad . \quad . \quad (25)$$

$$E = 1 + \frac{k'^2}{2}\left(\log \frac{4}{k'} - \tfrac{1}{2}\right), \quad . \quad . \quad . \quad . \quad (26)$$

and these enable us to verify the above value of Ω. Thus for any point, P, in the plane of the circle and within its circumference, $\Omega = 2\pi$; and if in (24) we put $\theta = 0$, we get

$$\Omega = 2\pi - (1-E)\pi - \frac{R^2}{8a^2}\left(2K - E\right)\frac{\pi}{2}$$
$$= 2\pi,$$

as we see from (25) and (26), since, to the second order, $k'^2 = \frac{R^2}{4\alpha^2}$. Similarly for any point, P, in the plane of the circle and outside its circumference, $\Omega = 0$; and this we find to be the case by putting $\theta = \pi$.

To find the value of Ω in its final form in terms of R and θ, we must substitute the value of k' in K and E. Now

$$z = \frac{R}{\rho} = \frac{R}{2\alpha}\left\{1 + \frac{R}{2\alpha}\cos\theta + \frac{R^2}{8\alpha^2}(3\cos^2\theta - 1)\right\}, \quad . \quad (27)$$

and if we denote $\log \frac{8a}{R}$ by L, we have

$$K = L - \frac{R}{2\alpha}\cos\theta + \frac{R^2}{16\alpha^2}(L + 1 - 4\cos^2\theta), \quad . \quad (28)$$

$$E = 1 + \frac{R^2}{8\alpha^2}(L - \tfrac{1}{2}). \quad . \quad . \quad . \quad . \quad . \quad . \quad (29)$$

Now in (24) K occurs only in terms of the first and second order, and therefore its value need be taken to the first order only, $i. e.$

$$K = L - \frac{R}{2\alpha}\cos\theta; \quad . \quad . \quad . \quad . \quad (30)$$

and hence we have finally

$$\Omega = 2\pi - 2\left\{\theta + \frac{LR}{2\alpha}\sin\theta + \frac{R^2}{16^2}(6L - 5)\sin\theta\cos\theta\right\}, \quad (31)$$

which is the expression for the conical angle correct to the second order of small quantities.

As a test of the correctness of this value of Ω we should find that $\nabla^2\Omega = 0$, as far as quantities of the second order. To apply this test, express Ω in terms of the columnar coordinates of P. Let the distance of P from the central axis OV (fig. 2) be ζ. Then we should have

$$\frac{d^2\Omega}{dz^2} + \frac{d^2\Omega}{d\zeta^2} + \frac{1}{\zeta}\frac{d\Omega}{d\zeta} = 0,$$

or if $\zeta = \alpha - \eta$, where $\eta = QN$,

$$\frac{d^2\Omega}{dz^2} + \frac{d^2\Omega}{d\eta^2} - \frac{1}{\alpha-\eta}\frac{d\Omega}{d\eta} = 0.$$

Now the term in brackets in (31) is

$$\tan^{-1}\frac{z}{\eta} + \frac{Lz}{2\alpha} + \frac{z\eta}{16\alpha^2}(6L-5),$$

and, observing that $R^2 = z^2 + \eta^2$,

$$\frac{dL}{dz} = -\frac{z}{R^2}, \qquad \frac{dL}{d\eta} = -\frac{\eta}{R^2}, \qquad \frac{1}{\alpha-\eta} = \frac{1}{\alpha} + \frac{R}{\alpha^2}\cos\theta,$$

we find $\nabla^2\Omega = 0$.

Before obtaining the effect at P due to the whole current flowing through the cross-section, $pk'Bk$, it is necessary to express α, the radius of the circular filament through Q, in terms of a the radius of the filament through D. If $PD = m$, we have $\alpha = a + R\cos\theta - m\cos\phi$, where $\phi = \angle$ PDO; and to the same order of approximation as before, (31) becomes

$$\Omega = 2\pi - 2\left\{ \theta + \frac{LR}{2a}\sin\theta \right.$$
$$\left. + \frac{R\sin\theta}{16a^2}\left[(3-2L)R\cos\theta + 8(L-1)m\cos\phi\right] \right\}, \quad . \quad (32)$$

where $L \equiv \log_e \dfrac{8a}{R}$.

Resultant Conical Angle.—The element of area of the cross-section at Q being dS, the resultant conical angle subtended by the circuit at P is $\int\Omega dS$, *if the density of the current in the cross-section is assumed to be constant.* This we shall assume for the present. Let $\angle$ QPD $= \chi$, and take $dS = RdRd\chi$. Then the first two terms in (32) will give $2(\pi-\theta)dS$, or $2(\pi-\phi+\chi)dS$, the integral of which is

2 D 2

$2(\pi - \phi) \cdot A$, where A is the area of the cross-section, $pk'Bk$. The term $\int \chi dS$ obviously vanishes, since χ is negative for points Q on the lower side of the line PD.

If the tangent from P to the circle $pk'Bk$ makes the angle ω with PD, the values of χ run from $-\omega$ to ω; and it is obvious any integral of the form

$$\int_{-\omega}^{\omega} f(\cos \chi) \sin \chi \, d\chi$$

vanishes, f being any rational symbol of functionality, while

$$\int_{-\omega}^{\omega} f(\cos \chi) \cdot d\chi = 2 \int_{0}^{\omega} f(\cos \chi) d\chi.$$

Now, the values of χ being understood to range from 0 to ω, and θ being equal to $\phi - \chi$,

$$\int \Omega dS = 2(\pi - \phi)A - \frac{2 \sin \phi}{a} \iint LR^2 dR \cdot \cos \chi \, d\chi$$

$$- \frac{\sin 2\phi}{4a^2} \left[\frac{3}{2} \iint R^3 dR \cdot \cos 2\chi \, d\chi - \iint LR^3 dR \cdot \cos 2\chi \, d\chi \right.$$

$$\left. + 4m \iint LR^2 dR \cdot \cos \chi \, d\chi - 4m \iint R^2 dR \cdot \cos \chi \, d\chi \right]. \quad (33)$$

If we first perform the integration in each case with respect to R, taking χ constant, we shall have, for example,

$$\iint R^3 dR \cdot \cos 2\chi \, d\chi = \tfrac{1}{4} \int (R'^4 - R^4) \cos 2\chi \, d\chi, \quad . \quad (34)$$

where $R' = Pk'$ and $R = Pk$ (fig. 2); but since in integrating throughout the semicircle $pk'BkO$ the values of χ are repeated in the revolution of the radius-vector, Pk', the values of $d\chi$ being negative as the radius-vector revolves from the position of the tangent from P, it is clear that both terms in the integral at right side of (34) are included in the single expression

$$\tfrac{1}{4} \int R^4 \cos 2\chi \, d\chi,$$

R now being a radius-vector from P to a point on the circumference of the circle.

Similarly

$$\int_{0}^{\circ} LR^3 dR \cdot \cos 2\chi \, d\chi = \int (\tfrac{1}{4}LR^4 + \tfrac{1}{16}R^4) \cos 2\chi \, d\chi.$$

To calculate the double integrals involved in (33), we shall take as the independent variable the angle $k'\mathrm{D}p$, or ψ; and we have, if c is the radius of the cross-section,

$$\mathrm{R}^2 = m^2 + 2cm \cos \psi + c^2, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (35)$$

$$\mathrm{R}^2 d\chi = c(c + m \cos \psi) d\psi, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (36)$$

$$\mathrm{R}^2 \cos 2\chi = 2\mathrm{R}^2 \cos^2 \chi - \mathrm{R}^2 = m^2 + 2cm \cos \psi + c^2 \cos 2\psi. \quad (37)$$

Hence

$$\iint \mathrm{R}^3 d\mathrm{R} \cdot \cos 2\chi \, d\chi = \tfrac{1}{4}\int \mathrm{R}^4 \cos 2\chi \, d\chi$$

$$= \frac{c}{4} \int_0^{\pi} (c + m \cos \psi)(m^2 + 2cm \cos \psi + c^2 \cos 2\psi) d\psi = \tfrac{1}{2}\mathrm{A}m^2, \quad (38)$$

putting A for πc^2. Similarly

$$\iint \mathrm{R}^2 d\mathrm{R} \cdot \cos \chi \, d\chi = \tfrac{1}{2}\mathrm{A}m. \quad \cdot \quad \cdot \quad \cdot \quad (39)$$

To find the integrals which involve the logarithms, observe that by trigonometrical expansion

$$\log \mathrm{R}^2 = \log m^2\left(1 + 2\frac{c}{m}\cos \psi + \frac{c^2}{m^2}\right)$$

$$= \log m^2 + 2\left(\frac{c}{m}\cos \psi - \tfrac{1}{2}\frac{c^2}{m^2}\cos 2\psi + \tfrac{1}{3}\frac{c^3}{m^3}\cos 3\psi - \&\mathrm{c}.\right). \quad (40)$$

Hence

$$\iint \mathrm{L}\mathrm{R}^3 d\mathrm{R} \cdot \cos 2\chi \, d\chi$$

$$= \tfrac{1}{4}\int \mathrm{L}\mathrm{R}^4 \cos 2\chi \, d\chi + \tfrac{1}{16}\int \mathrm{R}^4 \cos 2\chi \, d\chi$$

$$= \frac{c}{4}\int_0^{\pi} \mathrm{L}(c + m \cos \psi)(m^2 + 2cm \cos \psi + c^2 \cos 2\psi) d\psi$$

$$+ \tfrac{1}{16}\int (c + m \cos \psi)(m^2 + 2cm \cos \psi + c^2 \cos 2\psi) d\psi$$

$$= \frac{\mathrm{A}}{8}\left(4m^2\mathrm{L} - 2c^2 + \frac{c^4}{3m^2}\right), \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (41)$$

where L stands now for $\log \dfrac{8a}{m}$. Finally

$$\iint \mathrm{L}\mathrm{R}^2 d\mathrm{R} \cdot \cos \chi \, d\chi = \frac{\mathrm{A}}{2}\left(m\mathrm{L} - \frac{c^2}{4m}\right). \quad \cdot \quad \cdot \quad (42)$$

(Of course the integrations in ψ are very simple, since $\int_0^\pi \cos n\psi \cdot \cos n'\psi \cdot d\psi = 0$, except when $n = n'$, and then $\int_0^\pi \cos^2 n\psi \cdot d\psi = \dfrac{\pi}{2}$. No term in ψ beyond $\cos 3\psi$ contributes to the integral (41).)

Substituting these values in (33), and denoting the resultant conical angle, $\int \Omega dS$, by Θ, we have

$$\frac{\Theta}{A} = 2(\pi - \phi) - \frac{\sin \phi}{a}\left(mL - \frac{c^2}{4m}\right)$$
$$- \frac{\sin 2\phi}{16a^2}\left\{(6L - 5)m^2 - c^2\left(1 + \frac{c^2}{6m^2}\right)\right\}. \quad (43)$$

This result is susceptible of verification thus. If the components of magnetic force at P per unit pole per unit current are X, Z parallel to OD, OV, we have

$$\frac{dX}{dx} + \frac{dZ}{dz} + \frac{dY}{dy} = 0,$$

where $\dfrac{dY}{dy}$ is the line rate of increase of force at P perpendicular to the plane, POD, of xz. Y is, of course, zero, but we easily see that

$$\frac{dY}{dy} = \frac{X}{a - m\cos\phi},$$

by taking a position of the point, or pole, close to P along the perpendicular at P to the plane xz. Hence we should have, up to and including terms in $\dfrac{1}{a^2}$, the result

$$\frac{dX}{dx} + \frac{dZ}{dz} + X\left(\frac{1}{a} + \frac{m}{a^2}\cos\phi\right) = 0. \quad . \quad . \quad (44)$$

The values of X and Z are best found from $\dfrac{d\Theta}{dm}$ and $\dfrac{d\Theta}{md\phi}$, which (neglecting A for the moment) are the components of force along and perpendicular to DP.

Now

$$\frac{d\Theta}{dm} = -\frac{\sin\phi}{a}\left(L-1+\frac{c^2}{4m^2}\right) - \frac{\sin 2\phi}{16a^2}\left\{(12L-16)m+\frac{c^4}{3m^3}\right\}, \quad (45)$$

$$\frac{d\Theta}{md\phi} = -\frac{2}{m} - \frac{\cos\phi}{a}\left(L-\frac{c^2}{4m^2}\right) - \frac{\cos 2\phi}{16a^2}\left\{(12L-10)m-\frac{2c^2}{m}-\frac{c^4}{3m^3}\right\}; \quad (46)$$

also
$$X = -\frac{d\Theta}{dm}\cos\phi + \frac{d\Theta}{md\phi}\sin\phi, \quad \ldots \ldots \quad (47)$$

$$Z = \quad \frac{d\Theta}{dm}\sin\phi + \frac{d\Theta}{md\phi}\cos\phi, \quad \ldots \ldots \quad (48)$$

and
$$\frac{d}{dx} = -\cos\phi\,\frac{d}{dm} + \frac{\sin\phi}{m}\,\frac{d}{d\phi},$$

$$\frac{d}{dz} = \quad \sin\phi\,\frac{d}{dm} + \frac{\cos\phi}{m}\,\frac{d}{d\phi}.$$

The necessary condition (44) is found to be satisfied both for the terms of the first and for those of the second order.

Variable Current-density.—The preceding investigation assumes the current-density to be constant at all points in the cross-section of the wire. If, however, we assume that at any point, Q (fig. 2), it varies inversely as the distance of the point from the axis OV of the current, its value at Q may be written $\sigma\frac{a}{\alpha}$, where σ is a constant and a and α have the meanings already given to them[*]. If we put, as before, i for the total current traversing the section, we have

$$\sigma a\int\frac{dS}{\alpha} = i, \quad \ldots \ldots \ldots \quad (48\,a)$$

which gives $\sigma = \frac{i}{A}$, where A is the area of the cross-section. The resultant conical angle subtended now at P will be

$$\frac{i}{A}\int\Omega\,\frac{a}{\alpha}dS, \quad \ldots \ldots \ldots \quad (48\,b)$$

where Ω has the value given in (32) ; and since, to the second order,

$$\frac{a}{\alpha} = 1 - \frac{1}{a}\,(R\cos\theta-m\cos\phi) + \frac{1}{a^2}(R\cos\theta-m\cos\phi)^2, \quad (48\,c)$$

[*] The necessity for considering this case was pointed out to me by Professor Perry.

to the expression for $\int \Omega dS$ given in (43) must be added the correction

$$\int \Omega \left\{ -\frac{1}{a}(\mathrm{R}\cos\theta - m\cos\phi) + \frac{1}{a^2}(\mathrm{R}\cos\theta - m\cos\phi)^2 \right\} dS. \ (48\,d)$$

The term of the second order in Ω in (32) will, of course, contribute nothing to this correction, while the term of the first order in Ω is to be taken with the term

$$-\frac{1}{a}(\mathrm{R}\cos\theta - m\cos\phi)$$

only, so that the expression for the correction is

$$2\int (\pi - \theta) \left\{ -\frac{1}{a}(\mathrm{R}\cos\theta - m\cos\phi) + \frac{1}{a^2}(\mathrm{R}\cos\theta - m\cos\phi)^2 \right\} dS$$
$$+ \frac{1}{a^2}.\int \mathrm{LR}\sin\theta(\mathrm{R}\cos\theta - m\cos\phi)dS. \quad . \quad (48\,e)$$

Putting $\theta = \phi - \chi$, as before, the only terms of new form that present themselves are

$$2\iint \left\{ -\frac{\sin\phi}{a}\left(1 + \frac{2m}{a}\cos\phi\right). \mathrm{R}\chi\sin\chi \right.$$
$$\left. + \frac{\mathrm{R}^2}{2a^2}\sin 2\phi.\chi\sin 2\chi \right\} \mathrm{R}d\mathrm{R}d\chi ;$$

and it will be found that

$$\iint \mathrm{R}^2 d\mathrm{R}.\chi\sin\chi d\chi = \tfrac{1}{3}\int \chi(\mathrm{R}'^3 - \mathrm{R}^3)\sin\chi d\chi = \mathrm{A}\frac{c^2}{8m}, \quad . \quad (48\,f)$$

$$\iint \mathrm{R}^3 d\mathrm{R}.\chi\sin 2\chi d\chi = \tfrac{1}{4}\int \chi(\mathrm{R}'^4 - \mathrm{R}^4)\sin 2\chi d\chi$$
$$= \mathrm{A}\frac{c^2}{4}\left(1 - \frac{c^2}{6m^2}\right), \ (48\,g)$$

in which χ has been taken from 0 to ω only, so that in the correction $(48\,e)$ these must be doubled.

Hence the whole of the correction $(48\,e)$ amounts to

$$-\frac{\sin\phi}{2a}\frac{c^2}{m} + \frac{1}{16a^2}\left\{ 8c^2(\pi - \phi) - \left(2c^2 + \frac{2c^4}{3m^2}\right)\sin 2\phi \right\},$$

so that (43) is replaced by

$$\frac{\Theta}{\mathrm{A}} = 2(\pi - \phi) - \frac{\sin\phi}{a}\left(m\mathrm{L} + \frac{c^2}{4m}\right) + \frac{1}{16a^2}\left\{ 8c^2(\pi - \phi) \right.$$
$$\left. - \left[(6\mathrm{L} - 5)m^2 + c^2 + \frac{c^4}{2m^2}\right]\sin 2\phi \right\}, \quad (48\,i)$$

the right-hand side being the value of the resultant conical angle subtended by the circuit at the point P. The magnetic potential at P is therefore this right-hand side multiplied by i, the total current traversing the cross-section of the wire.

It will be found that this value of Θ satisfies, both for the terms of the first order and for those of the second, the condition (44).

If the density of the current at any point Q in the cross-section be assumed to vary as any power of the distance of Q from the axis OV, the conical angle subtended at a point near the wire is found just as easily as in the case in which the density is supposed to vary inversely as the distance. Thus, if it is proportional to $\dfrac{1}{a^n}$, (48 c) will be replaced by

$$1 - \frac{n}{a}(\mathrm{R}\cos\theta - m\cos\phi) + \frac{n(n+1)}{2a^2}(\mathrm{R}\cos\theta - m\cos\phi)^2,$$

and we have merely the same terms (48 f), &c., as before.

We see then that when small quantities of the first order, indicated by the fraction $\dfrac{m}{a}$, are taken into account, the magnetic potential, and therefore the magnetic force, at any point are not the same as if the whole current were condensed into an infinitely thin filament traversing the centre of the wire, D ; for, at points, P, close to the wire $\dfrac{c}{m}$ is nearly unity, and hence the fraction $\dfrac{c^2}{4m^2}$ is not negligible in comparison with L, unless, indeed, $8a$ is many thousands of times greater than c.

Consider the magnetic force, Z_E, just outside the wire at E. This is obtained by putting $\phi = 0$ and $m = c$ in $\dfrac{1}{\mathrm{A}}\,\dfrac{d\Theta}{m\,d\phi}$; thus, omitting the factor i,

$$Z_\mathrm{E} = -\frac{2}{c} - \frac{1}{a}(\mathrm{L} + \tfrac{1}{4}) - \frac{c}{16\,a^2}(12\mathrm{L} + 1),$$

while by putting $\phi = \pi$, $m = c$, we obtain the force, Z_E, at B :

$$Z_\mathrm{B} = -\frac{2}{c} + \frac{1}{a}(\mathrm{L} + \tfrac{1}{4}) - \frac{c}{16\,a^2}(12\mathrm{L} + 1),$$

whereas the magnetic force at the centre, O, of the circle is of the order $\dfrac{2\pi}{a}$, and is therefore much less than the force close to the surface of the wire.

Lines of Magnetic Force.—The forms, or approximate equations, of the lines of magnetic force close to the wire may be determined to the second order of small quantities in like manner. Thus, in my previous paper on the Magnetic Field of a Circular Current (*ante*, p. 206) I have shown that if at any point in space in presence of a current running in an infinitely thin circular filament G is the vector potential due to the current, we shall have

$$\mathrm{G} \cdot \alpha = \text{constant}$$

along the line of force, where α is the distance of the point from the axis of the current (*i. e.*, the perpendicular to its plane drawn at its centre). In the case of a point so close to a wire that the current through its cross-section must be broken up into filaments (as in the previous calculation of Θ), the total vector potential at any point is $\int \mathrm{G} d\mathrm{S}$, and as α is the same for all the filaments, the equation of a line of force is

$$\int \mathrm{G} \cdot \alpha \cdot d\mathrm{S} = \text{constant}.$$

But (Phil. Mag. *ibid.*)

$$\mathrm{G} \cdot \alpha = \{(1 + k'^2)\mathrm{K} - 2\mathrm{E}\}\rho,$$

and the current-density in the filament through $d\mathrm{S}$ being first supposed constant and equal to $\dfrac{i}{\mathrm{A}}$, the equation of a line of force is

$$\frac{i}{\mathrm{A}} \int \{(1 + k'^2)\mathrm{K} - 2\mathrm{E}\}\rho d\mathrm{S} = \text{constant}. \quad . \quad . \quad (49)$$

With previous notation and approximation, we have

$$\mathrm{K} = \log \frac{8a}{\mathrm{R}} + \frac{\mathrm{R}\cos\theta - 2m\cos\phi}{2a}$$
$$+ \frac{\mathrm{R}^2(\mathrm{L} - 1 - 2\cos 2\theta) + 8\mathrm{R}m\cos\theta\cos\phi - 8m^2\cos^2\phi}{16a^2}, \quad (50)$$

$$\mathrm{E} = 1 + \frac{\mathrm{R}^2(2\mathrm{L} - 1)}{16a^2}, \quad . \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (51)$$

$$\rho = 2a\left(1 + \frac{R\cos\theta - 2m\cos\phi}{2a} + \frac{R^2\sin^2\theta}{8a^2}\right), \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (52)$$

$$k'^2 = \frac{R^2}{4a^2}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (53)$$

where $L \equiv \log \dfrac{8a}{R}$.   Hence (49) becomes

$$\frac{4ai}{A}\iint\left[1 + \frac{R\cos\theta - 2m\cos\phi}{2a} + \frac{R^2\sin^2\theta}{8a^2}\right]\left[L - 2 + \frac{R\cos\theta - 2m\cos\phi}{2a}\right.$$

$$\left. + \frac{R^2(L+1-2\cos 2\theta) + 8Rm\cos\theta\cos\phi - 8m^2\cos^2\phi}{16a^2}\right]R\,dR\,d\chi, \quad \cdot \quad (54)$$

it being understood that χ ranges from 0 to ω, and therefore ψ from 0 to π, when (as in the calculation of Θ) the independent variable is changed from χ to ψ.

In addition to the integrals (38), (39), (41), (42), the following are now required, and they are easily deduced like the others :

$$\iint L R^3\,dR\,d\chi = \frac{A}{8}\{L(2m^2 + c^2) - 2c^2\}, \quad \cdot \quad \cdot \quad (55)$$

$$\iint R^3\,dR\,d\chi = \frac{A}{4}(2m^2 + c^2), \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (56)$$

$$\iint L R\,dR\,d\chi = \frac{A}{2}L, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (57)$$

$$\iint R\,dR\,d\chi = \frac{A}{2}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (58)$$

where $L \equiv \log \dfrac{8a}{m}$.

With these values (54) gives as the equation of a line of force

$$\frac{L}{2} - 1 - \frac{\cos\phi}{4a}\left\{(L-1)m + \frac{c^2}{4m}\right\} + \frac{1}{16a^2}\left\{L\left(\frac{m^2}{2} + \frac{c^2}{4}\right)\right.$$

$$\left. + \frac{m^2}{2} - \frac{c^2}{4} - \cos 2\phi\left(L\frac{m^2}{2} - \frac{c^2}{4} - m^2 + \frac{c^4}{24m^2}\right)\right\} = \text{const.}, \quad .(59)$$

to the second power of the small quantity or $\dfrac{c}{a}$.

As a verification, the curve (59) should be at right angles to the curve $\Theta = \text{const.}$ given by (43), *i. e.*, if we calculate the value of $m\dfrac{d\phi}{dm}$ for each curve, the one should be the

negative reciprocal of the other, as far as small quantities of the second order. This is found to be the case; for, from (59) we have

$$\frac{dm}{md\phi} = \frac{m}{2a}\sin\phi\left(L-1+\frac{c^2}{4m^2}\right) - \frac{m^2}{8a^2}\sin 2\phi\left\{(L-2)^2 + \frac{c^2}{4m^2} - \frac{7}{48}\frac{c^4}{m^4}\right\},$$

which is $-\dfrac{md\phi}{dm}$ calculated from the equation $\Theta = $ const.

If we denote the ratio $\dfrac{c^2}{4m^2}$ by λ, the result (59) may be written

$$\tfrac{1}{2}L - \frac{m}{4a}(L-1+\lambda)\,.\,\cos\phi + \frac{m^2}{16a^2}\{L(\tfrac{1}{2}+\lambda)+\tfrac{1}{2}-\lambda$$
$$-(\tfrac{1}{2}L-1-\lambda+\tfrac{2}{3}\lambda^2)\cos 2\phi\} = \text{const.}\quad.\quad.\quad(60)$$

Variable Current-density.—Let the current-density now be supposed to vary inversely as the distance from the axis OV. If at any point P in space Γ is the vector potential due to a system of circular currents all having the same axis OV, and if x is the distance of P from this axis, the equation of a line of force is, as has been shown,

$$\Gamma x = \text{constant.}$$

And Γ is the resultant vector potential at P due to a system of currents running in filaments through the wire, the density of the typical current filament through Q (fig. 1) being $\dfrac{i}{A}\,.\,\dfrac{a}{\alpha}$; therefore

$$\Gamma = \frac{4i}{A}\int\frac{\alpha}{\rho k^2}\{(1+k'^2)K-2E\}\frac{a}{\alpha}\,dS;\quad.\quad.\quad(61)$$

and since x is the same for all the filaments, in taking Γx we may put x under the sign of integration, so that

$$\Gamma x = \frac{i}{A}\int\{(1+k'^2)K-2E\}\rho\frac{a}{\alpha}\,dS.\quad.\quad.\quad.\quad(62)$$

As we require only the terms to be added to (59) by the introduction of the term $\dfrac{a}{\alpha}$, we may keep only terms of the first order in the expressions for $(1+k'^2)K-2E$ and ρ. Thus

$$(1+k'^2)K-2E=L-2+\frac{R\cos\theta-2m\cos\phi}{2a},\quad.\quad(63)$$

$$\rho = 2a\left\{1+\frac{R\cos\theta-2\,m\cos\phi}{2a}\right\}.\quad(64)$$

Hence, neglecting the factor $\dfrac{2ai}{A}$, the correction introduced is

$$\iint \left\{ L - 2 + (L-1)\, \frac{R\cos\theta - 2m\cos\phi}{2a} \right\} \left\{ -\frac{R\cos\theta - m\cos\phi}{a} \right.$$
$$\left. + \frac{(R\cos\theta - m\cos\phi)^2}{a^2} \right\} R\, dR\, d\chi. \quad (65)$$

In this the term of the first order is

$$-\frac{1}{a} \iint (L-2)\,(R\cos\theta - m\cos\phi)\, R\, dR\, d\chi, \quad \cdot \cdot \quad (66)$$

which, by putting $\cos\theta = \cos\phi\cos\chi$, is simply $A\,\dfrac{\cos\phi}{a} \cdot \dfrac{c^2}{8m}$.

The term of the second order is

$$\frac{1}{2a^2} \cdot \iint \{ (2L-4)\,(R\cos\theta - m\cos\phi)^2$$
$$- (L-1)\,(R^2\cos^2\theta - 3mR\cos\theta\cos\phi + 2m^2\cos^2\phi) \} R\, dR\, d\chi, \quad (67),$$

$$= \frac{1}{2a^2} \cdot \iint \{ (L-3)\,R^2\cos^2\theta - (L-5)mR\cos\theta\cos\phi$$
$$- 2m^2\cos^2\phi \} R\, dR\, d\chi, \quad (68)$$

which, by putting $\cos^2\theta = \tfrac{1}{2} + \tfrac{1}{2}\cos 2\phi\cos 2\chi$, &c., reduces to

$$\frac{A}{32a^2} \left\{ -L(2m^2 - c^2) - 7c^2 + \cos 2\phi\left(-c^2 + \frac{c^4}{3m^2} \right) \right\}. \quad (69)$$

[To prevent error, it may be observed that it is not allowable to put $\cos\theta = \cos\phi\cos\chi$ in any of the equations (63), (64), or (65) until, as in (66) or (67), all multiplications introducing powers of $\cos\theta$ have been performed.]

The terms (66) and (69), after neglecting the factor A, when added to (59) give as the equation of a line of force near the wire,

$$\frac{L}{2} - \frac{\cos\phi}{4a}\left\{ (L-1)m - \frac{c^2}{4m} \right\} + \frac{1}{16a^2}\left\{ L\left(\frac{3c^2}{4} - \frac{m^2}{2} \right) \right.$$
$$\left. + \frac{m^2}{2} - \frac{15c^2}{4} - \cos 2\phi\left(L\frac{m^2}{2} + \frac{c^2}{4} - m^2 - \frac{c^4}{8m^2} \right) \right\} = \text{const.} \quad (70)$$

As before, we can verify this result by showing that the curve (70) is at right angles to the curve (48 i). Taking, as previously, $\dfrac{c^2}{4m^2} = \lambda$, this equation is

$$\tfrac{1}{2}L - \frac{m}{4a}(L-1-\lambda)\cos\phi + \frac{m^2}{16a^2}\{ (3\lambda - \tfrac{1}{2})L + \tfrac{1}{2} - 15\lambda$$
$$- (\tfrac{1}{2}L - 1 + \lambda - \tfrac{1}{2}\lambda^2)\cos 2\phi \} = \text{const.} \quad (71)$$

It is interesting to observe that the supposition of variable density affects the term of the first order both in the value of Θ and in the constant of the line of force in the same way.

To trace any Line of Force.—With the centre, D, of the cross-section of the wire describe a series of circles, fig. 3, their radii being Df, Dg, Dh, . . .   Then to trace the particular line of force which passes through f (suppose) let D$f = m_0$, and let m be the radius of any other of the circles. If ϕ is the angle defining the point in which this latter circle is met by the line of force through f, we equate the left-hand side of (60) in case of constant density, and of (71) in case of variable density, to the expression which this left-hand side assumes when m_0 and 0 are put for m and ϕ.   Firstly, neglecting the terms of the second order, we have, to determine ϕ, in the case of constant density,

$$\tfrac{1}{2}L - \frac{m}{4a}(L - 1 + \lambda)\cos\phi = \tfrac{1}{2}L_0 - \frac{m_0}{4a}(L_0 - 1 + \lambda_0);$$

and if ϵ is the value of ϕ given by this equation, we can put $2\phi = 2\epsilon$ in the terms of the second order.   If the term of the second order, $\dfrac{m^2}{16a^2}\{. \ . \ .\}$, in (60) is denoted by γ, the more correct value of ϕ is obtained from the equation

$$\frac{m}{4a}(L - 1 + \lambda)(\cos\phi - \cos\epsilon) = \gamma - \gamma_0,$$

where in γ we put 2ϵ for 2ϕ, and in γ_0, of course, $2\phi = 0$.

As a numerical example, let the wire in fig. 1 form a circle 20 millim. in diameter, *i. e.* OD in fig. 2 is 10 millim.; and let the diameter of its cross-section be 2 millim. *i. e.* DB = 1 millim. in fig. 2.

In fig. 3 let EHB represent half the cross-section of the wire, its centre being D, and let the centre, O (see fig. 1), be in the production of the line DE at a distance 10 millim. above D, while DE = 1.   Let a series of circles be described round D with radii Df, Dg, Dh, . . . equal to 1·1, 1·2, 1·3, 1·4, 1·5 millim., and suppose that we trace the line of force which touches the wire at E.   If we calculate the angle ϕ

which defines the point in which this line cuts the circle of radius Df, we find, taking only terms of the first order, $\epsilon = EDp'$, or

$$\epsilon = 63° \; 21'.$$

This gives the point p'; but taking the terms of the second order the angle becomes $62° \; 37'$, which gives the true point,

Fig. 3.
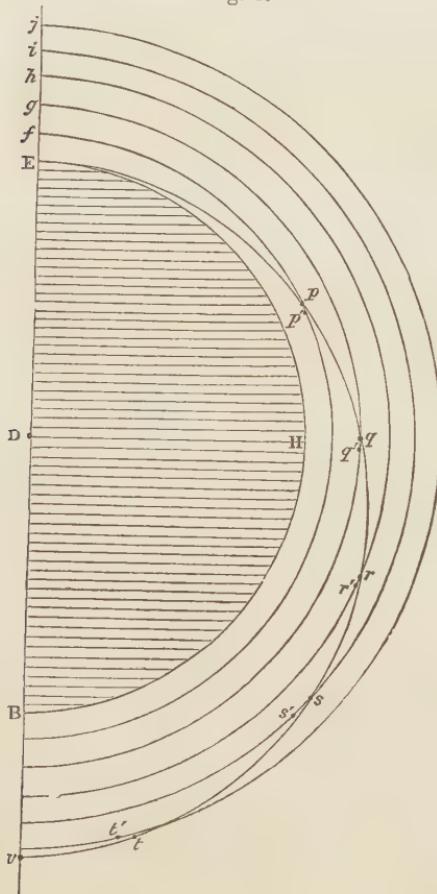
p, by means of the angle EDp. Similarly the point, q', in which, if only terms of the first order were taken account of, the line of force would cut the next circle (of radius Dg) is

defined by the value $\epsilon = 90° \, 14'$, which, being corrected by
the terms of the second order, becomes $89° \, 9'$, and the cor-
responding point is q.

In like manner the points r', s', t' given by terms as far as
the first order correspond to values of ϵ equal to

$$112° \, 48' \; ; \; 134° \, 26' \; ; \; 165° \, 41',$$

which are corrected into

$$111° \, 40' \; ; \; 133° \, 13' \; ; \; 163° \, 17',$$

the corresponding points being r, s, t. Thus the line of force
which starts from the inner surface of the wire is $\mathrm{E}pqrst$, and
it is found to cut the diameter EB in a point v such that $\mathrm{D}v$
is about 1·52 millim.

The lines of force at points between B and v are incomplete
curves which emanate from various points on the surface
EHB of the wire between E and B. The line of force at B
itself reduces to a mere point. These lines can, of course, be
traced by putting m_0 and π for m and ϕ in (60).

It has been already pointed out that the magnetic effect of
a current running in a wire is not the same, at points near
the surface of the wire, as if the whole current were con-
centrated in an infinitely thin filament running along the
central line of the wire, although such is sometimes assumed
to be the case.

Let us, for example, see what the values of ϕ, or of ϵ,
would be in the numerical case just discussed if we assume
that c can be put equal to zero, i. e. $\lambda = 0$.

The value of ϵ which corresponds to the circle of radius
1·1 millim. (supposing still that we are tracing the line of
force which passes through E) is found to be $66° \, 15'$ instead
of $63° \, 20'$; the value of ϵ which corresponds to the circle of
radius 1·4 is $142°$ instead of $134° \, 26'$, while that which cor-
responds to the radius 1·5 is impossible—indicating that, to
the first order of small quantities, the line of force does not
intersect the circle of radius $\mathrm{D}j$, but lies within it. Thus
there is a notable difference made in the results by assuming
that the whole current can be concentrated in a line running
through D.

If $a = \infty$, i. e., if the current runs in a straight wire, the
conical angle and the constants of the lines of force are the

same as if $c=0$, and therefore *such concentration of a current along the central line of the wire is allowable only when the wire is straight, or when the curve into which it is bent has a very large radius of curvature.*

In the case of variable current-density the equation determining ϵ is

$$\tfrac{1}{2}\mathrm{L}-\frac{m}{4a}(\mathrm{L}-1-\lambda)\cos\epsilon=\tfrac{1}{2}\mathrm{L}_0-\frac{m_0}{4a}(\mathrm{L}_0-1-\lambda_0);$$

and applying this to the same numerical case, we find that p', q', r', s' are determined by the angles

$$68^\circ\ 49';\quad 98^\circ\ 10';\quad 123^\circ\ 4';\quad 152^\circ\ 4',$$

while the position of t' becomes imaginary. This shows that the supposition of variable density brings the lines of force closer to the surface, B, of the wire—as we should expect *à priori*. It is not considered necessary to draw a separate figure for the case of variable density, since the forms of the lines of force and the method of drawing are sufficiently illustrated by the case of assumed constant density.

Initial or rapidly alternating Currents.—The magnetic potential and the forms of the lines of force will not be the same when the current has become steady as they were in the initial stage of the current, because, just at starting, the current is confined to the surface of the wire. If we can assume that when the current is entirely superficial its density (or its infinitesimal depth below the surface of the wire) is constant, the magnetic potential at any point P and the constant of the line of force can be obtained by subtracting from the value of the potential in (43) its value when c is replaced by $c-\Delta c$, and a similar subtraction from (59). Thus, the magnetic potential is the right-hand side of (43) multiplied by i, the total current in the cross-section. If δ is the density of this current, $i=\pi c^2\delta$, and if q is the total superficial current ($=2\pi c\delta\,.\,\Delta c$), the magnetic potential becomes

$$q\left[2(\pi-\phi)-\frac{\sin\phi}{a}\left(m\mathrm{L}-\frac{c^2}{2m}\right)\right.$$
$$\left.-\frac{\sin 2\phi}{16a^2}\left\{(6\mathrm{L}-5)m^2-c^2 2+\left(\frac{c^2}{2m^2}\right)\right\}\right].$$

From (49) it appears that the constant of the line of force

due to the current in the whole cross-section is (59) multiplied by i, or by $\pi c^2 \delta$. When this multiplication is effected, the differentiation with respect to c is to be performed, as in the case of the potential.

Of course the preceding discussion applies to the case of a circular vortex ring in a perfect fluid, the velocity of a particle at any point of the fluid being the analogue of the magnetic force. See Basset's 'Hydrodynamics,' vol. ii. chap. xiv. Mr. Basset assumes that for a vortex ring the magnitude of the cross-section is negligible, so that the concentration of the ring along its core filament is allowable; but, as we have seen, this requires that the ring must be an extremely thin one, and in most cases of the coiling of wires for the conveyance of currents the assumption would be inadmissible; for, the ideal vortex filament required by the assumption (infinitely thin as compared with the radius of its aperture) finds but a very coarse representative in any coil of wire. We have already given the values of the magnetic force at the centre, O, of the curve formed by the wire and at the points, E, B, just outside its surface; these are the analogues of the velocity of the (irrotationally moving) fluid at these points in the case of a vortex ring. The velocity with which the ring itself moves forward is given by Mr. Basset as equal to $\dfrac{f}{2\pi a}$ (log $\dfrac{8a}{c} - 1$), where f is the strength of the vortex (product of the cross-section and molecular rotation), while the velocity of the fluid at O is $\dfrac{f}{a}$; so that the ratio of the forward velocity of the ring itself to the velocity of the fluid at the centre, O, of its aperture is

$$\frac{\log \dfrac{8a}{c} - 1}{2\pi},$$

which, as Mr. Basset says, is "large" in the case supposed (*i. e.*, a very thin ring). We must observe, however, when comparing actual electrical coils with fine vortex rings, that for a vortex ring for which a is 1000 times c (which would ordinarily be considered as a "fine" ring), this ratio is not

very large: it amounts only to 1·27 ; while for a vortex ring the radius of whose aperture is 100 times that of its cross-section, this ratio is only ·9, *i. e.*, the ring moves more slowly than the fluid at its centre.

XXV. *A New Form of Influence-Machine.*
By JAMES WIMSHURST*.

IN April 1891 I had the honour to submit to this Meeting a very useful form of experimental Influence-machine, by means of which I was able to show that almost every combination of glass and metal, and also that plain glass disks, when moved and suitably touched, were capable of producing a flow of electricity.

It is one of those combinations, somewhat modified and extended, which I have now the further pleasure of bringing to your notice.

The machine consists of two disks of plate-glass, each of 3 ft. 5 in. diameter and $\frac{1}{4}$ inch thickness, mounted about $\frac{3}{4}$ inch apart on one boss and spindle. This spindle is driven by means of a handle, and the disks rotate in one direction.

In the space between the disks are fitted four vertical slips of glass, two being situated on the right-hand side of the machine and two on the left-hand.

The vertical edges of the slips which come nearest the spindle are cut to an angle, leaving a rather wider opening at the circumference than at the centre of the disk. Upon each slip is a brush, A, and also an inductor, the brush and the inductor being metallically connected ; the brushes are made of fine wire, and touch very lightly on the inner surfaces of the disks. The glass slips slide into place by means of suitable grooves at their top and bottom ends ; they may be removed and replaced readily at pleasure.

The essential parts are fitted together within a glass case, and in all respects the machine is so constructed as to be useful for experimental work. The limited size of the case causes the insulating distances between part and part to be small, hence the length of the sparks is reduced. All the

surfaces are coated with shellac, and when the brushes are
new and in proper condition the machine is self-exciting.

The charges are not subject to reversal when the terminals
are opened beyond the striking distance, for then the whole
of the induced charges pass by way of the neutralizing
brushes C, C. Another feature is that the neutralizing cur-
rent may also be broken without reducing the excitement,
but then the charges alternate from positive to negative with
each half revolution of the disks.
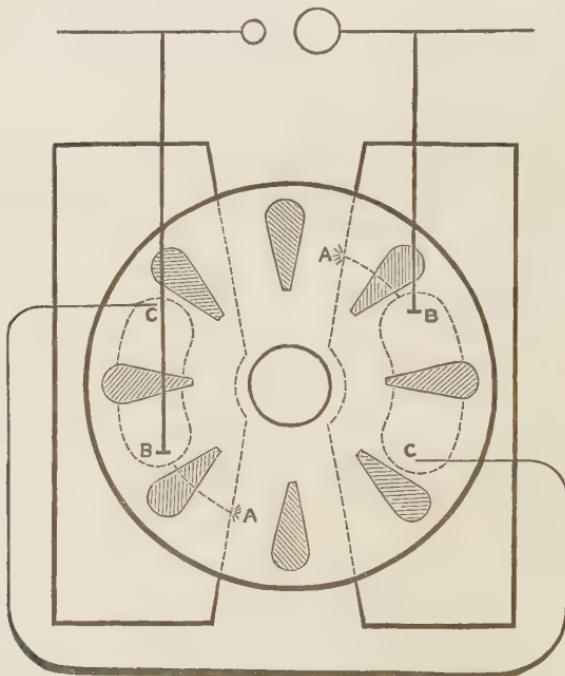
A, A. Brushes connected metallically to the inductors.
B, B. Collecting-brushes connected with terminals.
C, C. Brushes connected with the neutralizing circuit.

An approximate measure of the efficiency of the machine
is seen by the following experiments.

The glass throughout was held free from electrical excite-
ment, and the disks were rotated sixty times per minute ; the
actuating cause was then removed, and the disks came to rest
after making forty-seven revolutions. This measures the
friction of the machine.

The disks were then similarly turned, but the electrical charges were allowed to collect ; it was then found that the disks came to rest after twenty-three revolutions. The friction of the machine, therefore, is about equal to the repulsion of the electrical charges.

Leyden jars having eight square inches of tinfoil in each coating were then connected to the terminals, and the terminals set $3\frac{1}{2}$ in. apart : the disks were turned to the same number of revolutions and then left to come to rest, which they did after producing eighty sparks.

The disks were then turned and the number of sparks counted in relation to each revolution. This gave seven sparks of 1 in. length, five of 2 in., four of 3 in., and three of $4\frac{1}{2}$ in. length.

Eight metal sectors were then added (total 112 square inches on each disk) : with them the self-excitement becomes so free that, before any circuit was made, presenting the fingers to the disks caused electrical discharges.

When the circuits are made between the several brushes the disks appear to be seething with electricity, and the charges shoot out for a considerable part of their circumference, but chiefly at the top and the bottom, where they are not covered by the vertical glass slips, for the change of the charges from + to − takes place upon the disks just as they pass the edge of the slip. Improvements in this respect might be made, but the present arrangement has the advantage of giving permanence to the respective charges.

It will be noticed that strips of tinfoil are placed over the receding surfaces of the glass slips. They were intended to afford the means for collecting and carrying to earth any part of the charges from the inductors, but the earth connexion has not been necessary.

When at work the charges passing from the disks to the brushes may be heard some considerable distance; they resemble the beating of the floats of a paddle-wheel.

The charges are, however, reduced about 25 per cent. by the addition of the sectors.

Further tests have been made by fixing a pulley, 3 in. in diameter, to the overhanging end of the spindle, and suspending therefrom by a cord a weight of 15 lbs. The fall of

this weight through three feet produced seventeen revolutions when the disks were not excited and ten revolutions when excited. The same Leyden jars were then connected, and it was found that the fall of the weight through three feet produced twenty-eight sparks of $3\frac{1}{2}$ in. length. Reducing this to the terms of work in relation to sparking length, and omitting friction of the machine, it will be seen that 1 lb. weight falling through rather less than twelve inches produces a spark of $3\frac{1}{2}$ in. length. It must not, however, be forgotten that a considerable amount of electricity is also passing by way of the neutralizing circuit.

XXVI. *An Influence-Machine.* By W. R. PIDGEON[*].

IF we follow the action of any single sector on a disk of one of Mr. Wimshurst's beautiful machines, we find it goes through the following electrical changes :—Suppose it

Section of Disk.
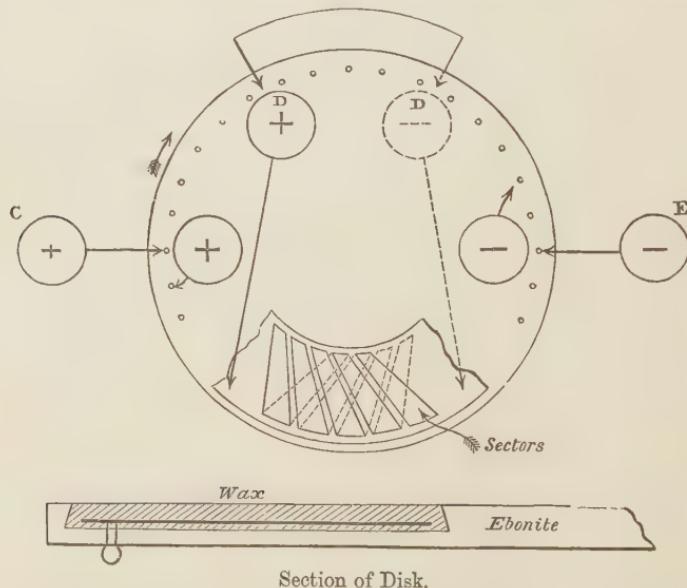
to be just leaving the positive collector C, it comes into a strong positive field produced by the other disk, and while

in this field is earthed by the first neutralizing brush D and
becomes charged negatively. Passing on it induces a posi-
tive charge on the other disk at the next neutralizing brush
D', and ultimately meets a similar negative charge, borne
forward by the other disk, at E, where it discharges into the
negative collector. I do not mean that this is a full explana-
tion of all that takes place, but I hope by its means to show
the lines along which I worked in designing the machine
before you. I thought that what had to be done was :—1st. To
make the capacity of each sector on the disk as great as
possible while being charged at D, and as small as possible
while being discharged at E, so that the amount of electricity
displaced by each sector should be as large as possible. 2nd.
To prevent any leak-back of electricity from sector to sector
as they are entering, or leaving, the different fields of induc-
tion. 3rd. To make the sectors large and numerous so as to
increase the capacity of the machine. And it was with these
objects in view that I built the machine which I will now
describe. The disks are made of ebonite $\frac{5}{8}$ inch thick and
are recessed as shown in the drawing, $\frac{3}{8}$ inch deep × 5$\frac{3}{8}$
inches wide. There are 32 sectors on each disk made of thin
sheet-brass, and having short brass wires soldered to them on
to which the small brass collecting-knobs are screwed. The
sectors were arranged in the recess as shown on the drawing,
the wires attached to them were passed through holes in the
disk, and the knobs screwed on from the back. The recess
was then filled up to slightly above the level of the face of the
disk with melted wax composed of equal parts by weight of
paraffin and rosin, and when cold the excess wax was turned off
in the lathe. Each sector is thus entirely imbedded in an
insulating material, the only exposed part, from which the
charge might brush back from sector to sector, being the
small brass knob. In order to minimize this possible leak the
sectors are set at an angle to any radius, as shown on the
drawing, so that a sector on one disk does not pass a sector on
the opposite one suddenly, but does so with a sort of shearing
motion requiring four times the angular movement of the disks
for the sectors to clear one another than would be needed
were they placed radially. By the time, moreover, that any
one sector has entirely entered a field of induction, the next

following sector is three parts into the field, and the next
behind that again is half in, while the third is one fourth the
way in, and the fourth just entering, so that the maximum
difference of potential between any two neighbouring knobs
is reduced to one fourth of what it would be were the sectors
arranged radially.   This angular arrangement of the sectors
naturally necessitates the collecting and neutralizing brushes
being displaced from the positions they would occupy were
the sectors radial so as to touch the latter at the right moment.
In order to increase the capacity of each sector while re-
ceiving its charge, two stationary inductors are placed opposite
each disk, one at each of the points where the sectors on that
disk are earthed.   Each inductor consists of a sheet of tin-
foil imbedded in wax and supported on a disk of ebonite, and
is charged from a pin-point connected to it through an
ebonite tube ; this pin-point stands opposite to the outer
knobs on another part of the large disk, where their sign and
potential are such as are required to charge the inductor.
Each sector at the moment of being earthed is thus placed
between two similarly charged inductors,—the sectors on
the opposite disk on the one side and the fixed inductor on
the other, each of which is large in comparison with itself ;
its capacity is thereby greatly increased and it is enabled to
carry forward a much larger charge of electricity than it can
when the stationary inductors are removed.

The effect produced on the output of the machine by using
these stationary inductors is remarkable, for though they
practically make no difference to the length of the sparks,
they greatly increase their apparent thickness ; and with
other things precisely similar they increase their frequency
three-fold.   To compare the output of the machine with and
without the stationary inductors, I counted the number of
sparks which overflowed a Leyden jar, and the revolutions of
the disks, for 30 seconds, both when using the inductors and
when they were taken off, and I was careful, in every case,
to keep the speed of the disks as nearly as possible constant.
I thus found that a 25-ounce Leyden jar overflowed 19 times
in 30 seconds without inductors and 54 times with them ; or,
making a slight correction for the revolutions of the disks
being less in the second case, the output was increased as

3 to 1 by the use of the inductors.   The explanation of this greatly increased output is, I think, to be sought in the fact that connecting the positive and negative terminals of this machine does not stop its action, as it does in the case of the Wimshurst machine, and that it is therefore ready to start sparking, with almost full vigour, the moment they are separated.   For when the inductors are removed, and two or three Leyden jars connected to the terminals, it is perfectly easy to note the discharge of the disks with each spark, and the time they take to recover, slowly at first and very rapidly at last, depending on the size of the condensers they are charging ; but when the fixed inductors are replaced the recovery is very much more rapid, especially in its early stages, owing I think to the fact that the inductors apparently do not lose their charge with each spark, and therefore, at once, start up the action of the machine.

With a view to decrease the capacity of each sector at the moment of its being discharged into the collector, I put on another set of inductors, one opposite to each of the main collecting brushes, and so arranged matters that they could be charged either from the neighbouring inductor or from the collector itself.   In this way each sector at the moment of discharging itself into the collector is almost surrounded by a field of the same sign as itself, and I hoped by this means to squeeze more of its charge out of it as it passed.   I was at first, however, disappointed to find that this second set of inductors made little if any difference to the output of the machine, whether they were connected to the other inductors or to the main collectors.   But on arranging them to collect their own charge directly from the small knobs on the revolving disks, at a point just before the main collecting-brushes, they became of value and increased the efficiency of the machine nearly $\frac{1}{5}$th.

By the kindness of Mr. Wimshurst and Dr. Lewis Jones I have been enabled to compare the output of my machine with that of a Wimshurst having eight 15-inch plates and also with one having two 16-inch plates.   I find that, area for area, and without using the stationary inductors, the efficiency of my machine is to that of the Wimshurst as 3 to 2 ; and with the inductors in use it is as 5·64 to 1.   That is, from 5 to 5$\frac{3}{4}$

times the area of plate is required to pass the collector in the
Wimshurst machine to that which must pass it in this
machine to produce the same result ; or, to give the figures
as we took them together :—

		sq. ft.
Mr. Wimshurst's 8 plate 15-in. diam. machine requires		97·07
of area to pass the collector per spark.		
Mr. Jones's 2 plate 16-in. diam. Wimshurst requires		87·83
W. R. Pidgeon's 4 plate 24-in. diam. machine, without inductors..		63·64
,, ,, ,, ,, with one set of inductors on		21·117
,, ,, ,, ,, with both sets of inductors on		17·2

I feel I ought to mention a mistake I fell into, while work-
ing on this machine, which nearly made me think that fixed
inductors were of no use. When first trying them I used
brushes, instead of a single point, to collect their charge, and
was disappointed to find the output of the machine reduced
to almost *nil*, because, as I ultimately discovered, the brushes
formed a bridge between the sector knobs and allowed the
charge to flow back from sector to sector, instead of being
carried forward to the collector. Another difficulty I have
not been able to get over, and one which greatly decreases
the actual output of the machine, is caused by the warping of
the ebonite disks, which, although $\frac{3}{8}$ inch thick, will not stand
true after being turned up, but move just sufficiently to throw
them out of balance and prevent their being driven at any-
thing approaching a high speed. The ebonite, moreover, is
not a sufficiently good insulator, and not only prevents my
getting a long spark but also reduces the possible efficiency
of the machine by becoming an increasingly good conductor
as the potential rises.

DISCUSSION.

In a written communication Prof. O. Lodge said his
assistant, Mr. E. E. Robinson, constructed a machine on lines
similar to Mr. Pidgeon's, a few months ago, and had now a
large one nearly completed. Mr. Robinson's fixed inductors
are carried on a third plate fixed between the two movable
ones. The sectors are quite small, and neither they nor the
inductors are embedded. On close circuit the machine gives

a large current ($\frac{1}{10000}$ ampere) and on open circuit exceedingly high potentials.   In Dr. Lodge's opinion Mr. Pidgeon attaches too much importance to his sectors and their shape.

Mr. J. Gray wrote to say that stationary inductors enclosed in insulating material would probably give trouble at high voltages, because of the surface of the insulator becoming charged with electricity of opposite sign to that on the inductor.   He suggested that this might explain why Mr. Pidgeon could not obtain very long sparks.

Prof. C. V. Boys enquired as to how far the wax made insulating union with the ebonite, for, if good, glass might possibly be used instead of ebonite.   He greatly appreciated the design of Mr. Pidgeon's machine.

After some remarks by the President, on the great advances which had been made, Mr. Pidgeon replied, and Mr. Wimshurst tried some further experiments with a small experimental machine.